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THE PSYCHOLOGICAL REVIEW

THE OBJECTIVES OF OBJECTIVE PSYCHOLOGY¹

BY L. R. GEISSLER

Randolph-Macon Woman's College

We have lately heard or read much about 'objectives.' We have been told, for example, that education must have a definite 'objective,' to be aimed at by all those who wish to participate in the "bio-social transformation of the animal-like infant into the civilized adult as we know him."² In particular our colleges and universities have been blamed for not pursuing the proper objectives in their curricula, and efforts have been made to establish such objectives by consensus of opinion of educators and administrators. Then, again, the writers of textbooks usually announce in their prefaces or introductions what their particular objectives are in writing the book. And so we seem to have become quite reconciled to the notion that every intelligent human being should set himself some definite objective.

Hence I decided, since I could not have my own way of abolishing the custom of presidential addresses, that I could at least have my own objective in obeying this custom. But as I know so little about the nature of objectives in human actions I decided that this would be a good reason for talking about it glibly, hoping that in so doing I might find out what is meant by the term. This may be a very

¹ Presidential address delivered at the twenty-fourth annual meeting of the Southern Society for Philosophy and Psychology, March 29th and 30th, 1929, University of Kentucky.

² A passage from A. P. Weiss quoted with implied approval by Max Meyer in PSYCHOL. BULL., 1927, 24, p. 376.

selfish reason, if you like, but it is nevertheless a good one and a modest one. For I might have had the much more ambitious but also more immodest objective of trying to enlighten your minds on some of the deep mysteries of consciousness or attention or other mental activity about which a man brought up in the introspectionist camp is supposed to have 'inside' information. Far be it from me to speak on such a subject on this occasion. It is not my objective to re-open a battle which according to our friend K. Dunlap³ was decided about two years ago by the arrival of a mutual and neutral friend called 'objective psychology,' a name which is to offend neither the behaviorist nor the introspectionist.

What is meant by the term 'objective psychology'? Is it really some new kind of psychology, or is it merely a new term for what we always have had with us? This new word has already received the distinction of being incorporated in the title of a recent textbook by another distinguished member of our Society.⁴ Here, then, I have found another topic about which my knowledge is very deficient, and so I decided to talk about it also with the same selfish objective of informing myself on this topic. You will understand now how and why I have chosen as my subject-matter for this occasion the strange combination expressed in the title 'The Objectives of Objective Psychology.'

The different questions concerning this topic which I shall try to answer for myself are: What is meant by objectives? What are the objectives of other sciences? What is meant by objective psychology? And, what are the objectives of the science of objective psychology?

Consulting the dictionary,⁵ I found no difficulty with the meaning of the term 'objectives.' It was originally used in military language to designate the distant point toward which the march of troops was directed. The limited food-supply along a single road or line of advance usually made it necessary

³ *Phil. Rev.*, 1927, 34, pp. 462 ff.

⁴ J. F. Dashiell, *Fundamentals of objective psychology*, 1928.

⁵ J. A. H. Murray, *A new English dictionary on historical principles*, 1909, Vol. 7, p. 17.

to divide the armed forces into several divisions which advanced along several converging lines meeting at the objective point whose possession seemed of importance to the leader of the military expedition. This military phrase was later taken over into the general literary style and became a synonym for aim or purpose. Perhaps even now the term 'objective' seems to have a more concrete meaning, referring to something that can be actually accomplished rather than an abstract ideal that may be striven for but never actually reached. But this implication may be surreptitious and due to another meaning of the word objective, namely that of referring to an external object as opposed to an internal state or condition. Furthermore, the term objective refers to something that is not accidentally achieved; on the contrary, it is definitely premeditated or anticipated, not only in its final outcome but also as to the steps necessary to accomplish the end-result.

For example, let us watch the actions of a chess-player, or of a landscape architect, or of a painter or any other person whose overt behavior is readily accessible. We can easily notice the temporal sequences of his actions, measure their speed and accuracy, observe their recurrence or persistence, etc., but their inter-relations and relations to the final end-result remain more or less obscure unless we know beforehand what their objective is. Now this understanding of the actor's objectives and of his general plans gives an entirely new and better meaning to his different movements. In fact, our knowledge of a man's objective makes his behavior an entirely different kind of behavior. It is this possible knowledge of a person's objectives which makes the study of human behavior absolutely different in kind from the study of mere animal behavior. This point has been overlooked by some behaviorists; but that is aside from our present topic.

How absurd must a football game appear to an onlooker who is ignorant of the objectives of the game! Here we have a number of individuals all working toward the same objective, but each doing something entirely different from everybody else. There is the general or remote objective

of making as many touch-downs as possible and preventing the opponents from scoring. But there are also various immediate objectives, such as keeping the ball in one's own possession as long as possible, of gaining as much ground in every attack as possible, of overcoming the resistance of the other team or of preventing their further progress by anticipating their next move, etc. These immediate objectives must have certain intimate relations to the ultimate or remote objective. The team's captain may form a certain plan and depend upon his team-mates to carry out their share in this plan according to the opportunities offered them, and final success depends upon the intelligent coöperation of all. Now the uninformed observer may actually observe more details than the informed onlooker, and yet fail to understand what it is all about.

Or let us take another example. A ship at sea has been unsuccessful in her battle with the storm and is sending out radio signals of distress. Another, larger boat, receives these signals and the captain decides to bend every effort to come to the rescue. Here is his remote objective. The immediate objectives concern themselves with locating the wrecked ship, overcoming the forces of wind and waves, working the engines to get their highest speed, and otherwise utilizing every available means and preparing for the final rescue and harboring of the shipwrecked crew. Perhaps again one man works out the entire plan, but he must communicate it to his officers and they to the rest of the crew in order to obtain their most strenuous and intelligent coöperation without which the remote object could not be obtained.

In quite a similar way scientists all over the world are battling with the forces of nature. Have they a common objective? Are they all working toward the same end, remote perhaps, but nevertheless definite? And what are their immediate objectives? Are they intimately related to each other? And are these scientists willingly and intelligently coöoperating with each other? Only a complete history of science can give the answers to these questions. At most we can give here but a few statements of the ultimate objectives

of science, as formulated by some of its most distinguished leaders.

The division of human knowledge into special sciences is usually attributed to Aristotle, who for this reason is sometimes referred to as the father of all sciences. He divided the field of human knowledge into philosophy or speculative science and into practical science. He says with regard to the objectives of these two general divisions, "of speculative science the end is truth, but of practical science, work, for even though practical scientists may examine how a thing is, they do not investigate the cause of the thing itself but in relation to something else and as connected with the present time."⁶ In present-day terminology this statement means that science is not concerned with ultimate causality but only with present and past relationships and functions.

The doors to the modern era of scientific progress were opened by the two great contemporaries of Shakespeare, Descartes and Bacon. Of these two men Boutroux⁷ says: "Bacon and Descartes regarded science as having for its object, to arrive at laws which should possess the dual characteristic of universality and reality." In other words, the objective of science according to these leaders in human progress, is to deduce from real or individual facts of observation the general laws or principles which govern the events of the whole universe. Again the problems of the ultimate nature of the universe are left to philosophy.

Coming down to modern times, we find the objectives of science clearly enunciated by several important writers. One of the most concise statements may be found in Karl Pearson's 'Grammar of Science' where he says, "the classification of facts, the recognition of their sequence and relative significance is the function of science,"⁸ and again, "When every fact, every present or past phenomenon of that universe, every phase of present or past life therein, has been examined, classified, and coördinated with the rest, then the mission of

⁶ The metaphysics of Aristotle, Tr. by John H. M'Mahon, 1912, Book 1, p. 48.

⁷ Emile Boutroux, Natural law in science and philosophy. Tr. by Fred Rothwell, 1914, pp. 11 f.

⁸ Grammar of Science, 3d edition, 1911, p. 6.

science will be completed;"⁹ but such a goal is incapable of attainment because, as he continues elsewhere, "the universe is a variable quantity which depends upon the keenness and structure of our organs of sense, and upon the fineness of our powers and instruments of observation."¹⁰

In short, observation, classification, inter-relation and interpretation of all the facts and events of the universe are the ultimate objectives toward which the scientific march of progress is directed. Toward this remote goal all the special sciences are converging along their own particular lines of advance, now slowly, now at a quickened pace, now in a straight and direct path, and now in a roundabout way; but always keeping the ultimate objective in sight.

Let us briefly review a few recent statements of these immediate objectives of various sciences, such as physics, geology, chemistry, and biology, as formulated by recognized authorities in each field of scientific research. Physics has been defined as the science of the behavior of inanimate nature. In a recent textbook by Millikan, Gale and Edwards,¹¹ we find the statement that "physics is essentially a system of explanations—answers to the question why?—of the behavior of inanimate things." Kimball, in his 'College Textbook of Physics,'¹² says: "It is the aim of physical science so to systematize our knowledge of the material world that all its phenomena shall be seen as special instances under a few far-reaching and more inclusive generalizations called laws. . . . In seeking an explanation we determine the *causes* of the phenomenon in question; that is, the *essential* circumstances or those circumstances without which the given event does not occur; and then we seek to determine the effect of each of these circumstances separately, and exhibit, if possible, each such effect as a special instance under some general law."

The objectives of geology are stated very clearly by Sir

⁹ *Ibid.*, p. 12.

¹⁰ *Ibid.*, p. 15.

¹¹ *A first course in physics for colleges*, 1928, p. 3.

¹² *Third, revised edition*, 1923, pp. 1f.

Archibald Geikie in the fourth edition of his 'Textbook of Geology'¹³ as follows:

Geology is the science which investigates the history of the earth. Its object is to trace the progress of our planet from the earliest beginnings of its separate existence, through its various stages of growth, down to the present condition of things. Unravelling the complicated processes by which each continent and country has been built up, it traces out the origin of their materials and successive stages by which these materials have been brought into their present form and position.

Nor does this science confine itself merely to changes in the inorganic world. Geology shows that the present races of plants and animals are the descendants of other and very different races that once peopled the earth. . . . The geographical distribution of existing faunas and floras are often made clear and intelligible by geological events; and in a similar way it throws light upon some of the remoter phases in the history of man himself.

A definition of chemistry and a statement of its objectives is given by Professor John Johnston of Yale in a recent work on 'The Development of the Sciences' by various authors and edited by L. L. Woodruff. To this work Professor Johnston contributes the chapter on Chemistry¹⁴ which he begins with the following statement:

Chemistry is the science of the ultimate composition and constitution of matter, of the mutual reaction between two or more substances, and of the influence of factors such as change of temperature, pressure, or extent of surface upon the stability of a substance and its relation to other substances. The chemist studies the great diversity of substances, organic and inorganic, which we see around us; he analyzes these substances, ascertains their composition, and builds them up again from their components; he investigates their behavior with respect to change in external conditions and in relation to other substances. He learns how, not merely to imitate a substance occurring naturally, but to make the identical material artificially and to discover new substances superior in usefulness to those found in nature; and he considers how useful substances may be produced more economically from the raw

¹³ Fourth revised and enlarged edition, 1923, p. 1.

¹⁴ Yale University Press, 1923, p. 75.

materials available. The study of chemistry is slowly yielding information as to the nature of biological processes of importance to everyone and so is assisting to retain health and to control disease.

As psychologists we are, however, most vitally interested in the objectives of the biological sciences, because of their intimate relation to our own science. Professor Rogers¹⁵ of Oberlin College quotes Jacques Loeb as summing up the aim of physiology in these words: "To visualize all phenomena of life in terms of groupings and displacements of ultimate particles." L. L. Woodruff of Yale concludes his 'survey of the epochs and epoch makers in biological progress' as follows: "The biological sciences are developing with amazing rapidity at the present time chiefly through the cumulative influence of an all-pervading desire of students of life phenomena to observe nature at work—actually to control and modify biological processes. . . . In a word, the modern biological ideal is to construct an account of the living organism which can be verified by actual observation provided the proper conditions are afforded. Biology has emerged from the phase of development in which the descriptive note was dominant and has become in fact an experimental science."¹⁶ And in another connection he says, "In one direction, supported by chemistry and physics, biology becomes bio-chemistry and bio-physics. In a contrary direction it forms a connection with the psychical sciences which relate to human nature, with psychology and sociology, with ethics and religion."¹⁷ He thus makes biology the connecting link between the natural sciences and the mental sciences.

It is not beside our problem to inquire a little further into the objectives of one of the special branches of biology, namely physiology, in order to discover to what extent, if any, its objectives overlap with those of psychology.

According to Charles G. Rogers, whose recent 'Textbook

¹⁵ Textbook of comparative physiology, 1927, p. 5.

¹⁶ The development of the sciences, edited by L. L. Woodruff, 1923, p. 259.

¹⁷ Foundations of biology, 2d edition, 1923, p. 5.

of Comparative Physiology' I have consulted, the science of physiology 'includes all the activities of the (living) organism under definite or changing environmental conditions—its reactions to its environment,'¹⁸ while Comparative Physiology, in particular, deals "with the workings of the vital machine, whether it be simple or complex, Amœba or man."¹⁹ According to this same author, the aims of Comparative Physiology are: "(1) To give an exact and adequate conception of the essential nature of the various processes of the organism; (2) To make a comparison of these processes in different types of animals to discover whether they really operate under the same fundamental laws; (3) To trace, if possible, the course of evolution of these processes from the more generalized to the more specialized and complex; (4) To gain some idea, from the similarity of method employed by different groups of animals, of the possible relationships existing between them; and (5) To show the correlation and integration of the various parts and processes in the economy of the individual."²⁰ As psychologists we are particularly interested in this fifth point, "the correlation and integration of the various parts and processes in the economy of the individual," because some psychologists claim this as the distinguishing feature between psychology and physiology, as I shall try to show later. As a matter of fact, the physiologist Rogers very characteristically spells amoeba with a capital 'A' and man with a small 'm,' which indicates where he places his emphasis, in comparison with those psychologists who wish to limit the subject-matter of psychology to physiological processes. Nevertheless, he does aim to study the "correlation and integration of the various parts and processes in the economy of the individual." If, according to some psychologists, psychology does not aim to do any more than that with reference to one of the living organisms only, man, it would then become a subdivision under a subdivision of physiology, which itself is but one branch of general biology.

¹⁸ Textbook of comparative physiology, 1927, p. 3.

¹⁹ *Ibid.*, p. 3.

²⁰ *Ibid.*, pp. 4f.

Although Aristotle, by his own example, had shown that the study of human nature must be pursued by the method of observation, in contrast with the method of the speculative sciences, nevertheless for more than two thousand years psychology remained the handmaid of philosophy, with no special objectives of its own. Whatever incidental observations of mental activities had been made and whatever theories about the human soul had been advanced, were intended to support some metaphysical speculation about the ultimate nature of reality, which Aristotle stated to be the problem of speculative philosophy. Even as late as about 150 years ago Kant²¹ denied the possibility of psychology ever becoming a natural science on the *a priori* ground that it dealt with phenomena of one dimension only which therefore could not be measured. This problem of measurement has remained a stumbling block even at the present writing. Since the pioneer work of Weber and Fechner and Helmholtz, psychologists have found many opportunities for exact measurement in their subject-matter; yet there remain many facts of psychological observation to which direct measurement cannot be applied, because they are subjective in their very nature and cannot be observed by anybody but the person who experiences them. What is to be done with this subjective material in our science? European psychologists evidently have not worried themselves much over this question, but in this country a group of psychologists have arisen who feel that the recognition of subjective facts is irreconcilable with scientific accuracy and measurement and should be excluded from the subject-matter of psychology. Whether we are to agree with this movement or not will depend upon our answers to the two questions: What is psychology? and, What are its objectives? We must therefore understand the full significance of these important questions.

The difficulty of defining psychology is in the last resort the same difficulty which faces every other scientist who wants to formulate an exact delimitation of the subject-

²¹ Metaphys. Anfangsgründe d. Naturwissenschaft, 1786, pp. xf.

matter of his science, namely, the lack of natural lines of sharp divisions in the universe. Many other sciences study human nature, and only artificial and arbitrary lines of subdividing this field can be drawn. We must therefore expect some overlapping in subject-matter and in problems. But if psychology is to maintain its autonomy among the various sciences of human nature, this overlapping should be reduced to the absolute minimum. Furthermore, if in the past psychology has appropriated for its own particular subject-matter certain phases of human nature that are not investigated by any other science, then these other sciences have a right to expect that psychology will in the future continue to study these phases, because of the light such study may throw upon the problems of these other sciences. The only way to escape this responsibility would be for psychology to split into two new sciences, one of which would confine itself to the behavioristic facts of human nature, the other to the conscious phases of it. This would require the drawing of new arbitrary and artificial lines of distinction and has been advocated by certain psychologists.

The desirability of such a step would have to be decided by reference to the ultimate objectives of all sciences. We would have to determine whether such a division in psychology would facilitate or hinder the general progress of science. If it is impossible to anticipate with any degree of certainty the answer to this question, then psychologists would be justified in attempting the division and awaiting its outcome. The deciding factor in this attempt would be whether the separation into two independent sciences, each with its own objectives, would leave a residue of certain problems about human nature which is not properly the subject-matter of either of the two new sciences. If so, then the division has produced a no-man's-land which would interfere directly with the progress of science in general. In other words, are there any aspects of human nature in which the interrelation of behavior and consciousness is so intimate and so absolutely essential that the artificial separation of the two would distort the facts and lead to erroneous

interpretations? The existence of such facts would necessitate a combination of the objective and subjective methods of observation and therefore defeat the attempt to divide psychology into two new sciences.

So far as my present lack of knowledge goes, no psychologist of any school has categorically denied the existence of such facts. As a rule, behaviorists have avoided the issue, sometimes by a shift of emphasis, sometimes by a change in terminology, but more often by simple omission and re-interpretation, as can be readily seen by reference to recent textbooks on psychology written from the behavioristic point of view.

The shift of emphasis is well illustrated in the text by Smith and Guthrie ²² from which I quote: "It must not be supposed that thinking is denied by physiological psychology. A behavioristic description of man's mind in no way contradicts the common sense assumption that men are conscious. We shall first find out what man *does*, and under what circumstances he does it, because this is open to observation and may be stated exactly. An understanding of behavior is essential to an understanding of consciousness." The reader might ask why is not an understanding of consciousness equally essential to an understanding of behavior? But now note the shift in emphasis: after the authors have devoted 239 pages to an understanding of behavior, they relegate the problems of consciousness to an appendix of 18 pages, for which I am not thankful. I would prefer Dashiell's complete turn-about in his misnamed text 'Fundamentals of Objective Psychology' where in his preface he admits that "on certain phases of the subject, indeed, where the processes involved are especially subtle and intricate, their accurate determination as physical processes may actually be furthered by the subject's effort to observe and to report on them. This is true especially in such behavior as attending and thinking"; ²³ yet he decided to limit himself entirely to the behavioristic approach because it has added an 'extra-organic objectivism'

²² General psychology in terms of behavior, 1921, pp. 1f.

²³ Fundamentals of objective psychology, pp. vff.

to the 'physiological or intra-organic objectivism' which was already accepted by 'even the most extreme subjectivists.'

An illustration on changes in terminology may be taken from 'The Psychology of the Other-One' by Max Meyer. This author warns us, for example, against the use of such words as 'hungry' and 'sensation,' because he rejects "all terms that have a subjective meaning, that refer to consciousness."²⁴ Nevertheless he elsewhere criticizes²⁵ Bentley for changing his terminology for the sake of greater clearness. Meyer substitutes for the word 'sensation' the term 'excitation' and then applies to it adjectives which characterize sensory experiences, thereby spoiling the whole effect. What, for example, is the meaning of a 'green excitation'? According to our author such a term is supposed to help us understand better the nervous activity involved in seeing green. One can understand what is meant by a 'nervous excitation'; but what else can be meant by such phrases as a 'blue excitation' or a 'C# excitation' or an 'asafetida excitation' and the like than a reference to the conscious experiences connected with these excitations? Furthermore, how do we know that a 'green excitation' as a pure neural process is different from a 'sour excitation' as a pure neural process? All we do know is that the two nervous excitations are originated in two different receptors and conducted along two different nerve-paths. If the author means to say visual excitations and auditory excitations, and the like, he is not doing anything new; neither does this help him to distinguish one visual excitation from another visual excitation, because such distinctions are conscious processes and not merely neural processes. He would be much more objective to qualify his excitations by reference to their corresponding stimuli, for example, as a $395 \mu\mu$ excitation or as a 50° centigrade excitation, etc. Nobody has as yet invented a complete terminology that avoids all reference to mental activity; and even if it were possible to construct such a new language, it would have to be learned and understood through the

²⁴ The psychology of the other-one, p. 17.

²⁵ *Phil. Rev.*, 1927, 24, p. 360.

medium of consciousness, because all communication requires not merely the behavioristic processes of signalling through spoken or written or gesture language, but also the reference to the past experiences of the individual who receives the signals. This factor is overlooked also by Watson in his attempt to explain language and thought as mere implicit language habits.

But before we take up this matter let me point out that the device of simple omission of certain important facts in behavioristic treatises is so obvious as to require no detailed illustrations. Such matters as feelings, imagery, meanings, relations, attention, and purpose are most embarrassing topics to treat exclusively as forms of behavior, unless they undergo considerable reinterpretation. According to 'The Psychology of the Other-One,' animals and human beings eat "because for some time no food has been taken into the digestive cavity" ²⁶ and not because they feel hungry. Now the fact is that human beings do sometimes feel hungry while at the same time their stomachs are empty; but sometimes they feel hungry even soon after eating, and sometimes their stomachs are empty and yet they do not feel hungry. Is not the feeling hungry just as good a response to an empty stomach as movements in search of food or crying for food? Besides, an empty stomach cannot become more empty but the feeling of hunger can become stronger and stronger and gradually exclude every other conscious experience and lead to temporary delirium. A hungry feeling human being may behave quite differently from a human being who does not feel hungry, irrespective of the contents of their stomachs. In such a case it is not the contents of their stomachs but the contents of their minds that makes the difference. Nor does this statement mean that mental contents are causes of behavior or behavior-changes; it merely does mean that behavior is not the only response made by the subject, that an important factor is the mental response, which needs investigation and must not be silently overlooked.

The same reinterpretation of facts by behaviorists is

²⁶ M. Meyer, *op. cit.*, p. 12.

illustrated in their treatment of language and thought as mere forms of behavior. According to Watson, thought "is not different in essence from tennis playing, swimming, or any other overt activity except that it is hidden from ordinary observation and is more complex and at the same time more abbreviated so far as its parts are concerned than even the bravest of us could dream of."²⁷ But what stuff are such dreams made of? And if it takes such day-dreams for a behaviorist to understand thoughts as forms of behavior, then the anti-behaviorist may surely be pardoned for his use of introspection as a means of studying the nature of thoughts. And yet Watson flatters himself or perhaps merely dreams that in this way he is putting "thought back upon reasonable and investigable grounds."²⁸ When it comes to the problem how the word-stimulus issued by one individual is understood by another individual, the behaviorist again has to reinterpret the fact by saying that it does not have to be understood at all but merely reacted to, because that is all he can observe by his so-called objective method. Therefore, meanings, relations, references, and the like can also be treated as forms of behavior—if you are willing to dream about them.

Enough has been said to show that there are certain aspects of human nature in which the interrelations of behavior and consciousness are so intimate and so essential to the facts involved that an artificial separation of the two distorts the facts and leads to one-sided interpretations or abstractions. We must therefore conclude that psychology should not split into two new sciences nor limit itself exclusively to either consciousness or behavior, because both are mere abstractions from the concrete events to be studied. I cannot therefore agree with Dashiell when he says, "A scientific study of man assumes that he is *a complex physical object moving in a world of physical energies and relationships*. Anything of psychological interest about man is to be treated as a physical phenomenon, in the broader sense of that term, as a natural occurrence in which material bodies effect energy

²⁷ Psychology from the standpoint of a behaviorist, 1919, p. 325.

²⁸ *Ibid.*, p. 325.

changes."²⁹ The trouble with human beings is that they are not simply chemically and physically reacting organisms; they are also consciously reacting organisms; and if we are not willing to deny this fact then we have no right to discard it. But entirely aside from this; the study of human beings as merely physically and chemically reacting organisms is already the subject-matter of one of the biological sciences, usually called human physiology, as I have already shown. How can psychology differentiate itself from this science? This difficulty has been recognized by some of our behavioristic friends, and they have insisted on a fine distinction which we need to examine a little further.

This distinction is most explicitly stated by Dashiell when he says, "The physiological descriptions of the phenomena (of behavior) are too piecemeal to satisfy the psychologist; he would seek descriptions of the behavior of the man (or animal) as a whole, especially in terms of his dynamic relations to the world about him":³⁰ and so he concludes his book with a chapter on personality where he says, "Each particular reaction-unit can function truly as an adjusting device only when it is in harmonious relations with all other reaction units. Each act can mean an adjustment only if and when it secures some adjustment of the organism as a whole to its conditions of life as a whole."³¹ The line of distinction, then, between the physiological and psychological study of man, according to behaviorists, lies in the phrase, 'man or animal as a whole,' instead of what? Would physiologists admit that they are not interested in the organism as a whole and treat merely individual parts and functions and isolated reactions? My previous quotation from Rogers contradicts such an assumption. And furthermore, from the standpoint of physical and chemical changes, is the whole more than the sum of its parts? Does the physicist treat moving bodies sometimes as parts and sometimes as wholes? If there is some special significance in the fact that human beings

²⁹ Fundamentals of objective psychology, p. 10.

³⁰ J. F. Dashiell, *op. cit.*, p. 13.

³¹ *Ibid.*, p. 549.

(and animals) can react as wholes, how can that be treated as a physical phenomenon "in which material bodies effect energy changes"? Such a distinction is either invalid or else implies a non-mechanistic conception. If invalid, then psychology has lost its autonomy and becomes human physiology; but if valid, then man cannot be *fully* understood scientifically as a mere mechanism, even though in him "the interlockings of part on part are infinitely more complicated and subtle."³²

A similar criticism must be made of the effort to place the distinction between physiology and psychology in the term 'social.' Are atoms, molecules, electrons, and the like, social in their reactions on each other? What mechanistic significance can there be in the term 'social behavior' other than mechanical reaction of one human being to another human being? Perhaps Weiss has felt this objection and tried to meet it by using the term 'bio-social.'³³ No, if psychology is to imitate the exact natural sciences, it must do as Dashiell says, treat man as a mere 'complex physical object moving in a world of physical energies and relationships,' in which social relationships are unknown because only conscious beings can be social beings. The term social has no significance except for beings who are conscious of fellow-beings. To be conscious of fellow-beings is not merely to be aware of their existence; it involves definite feelings and evaluating attitudes towards these beings and, most important of all, it involves interpreting their behavior in terms, not of one's own behavior, but of one's own consciousness. This is implied, whether inadvertently or not, in Dashiell's definition of personality, when he says: "A man's personality—we may conclude—is his system of reactions and reaction-possibilities *in toto* as viewed by fellow-members of society. It is the sum total of behavior trends manifested in his social adjustments."³⁴ There are two implications in this definition which contradict what he calls the scientific

³² Dashiell, *op. cit.*, p. 550.

³³ See footnote 2.

³⁴ Dashiell, *op. cit.*, p. 551.

or, better, mechanistic, study of man; one has been already pointed out before. It involves the illegitimate equating of the term 'sum total of behavior' with the term behavior 'in toto.' The other implied contradiction is concealed in the phrase 'as viewed by fellow-members of society.' A strictly mechanistic study of man cannot deal with 'fellow-members of society viewing each other.' Such conceptions belong only to a psychology which recognizes human beings as something more than merely infinitely complex mechanisms.

This kind of psychology has always existed, since Aristotle, and will continue to exist as an honorable member of the group which he called practical sciences and which we usually speak of as the natural sciences. It is only this kind of psychology which deserves to be called 'objective psychology,' because it alone studies the objective or concrete events occurring to human beings, while a purely introspective psychology as well as a purely behavioristic psychology deals only with abstractions; for human behavior without consciousness is as much an abstraction as consciousness without behavior. Psychologists of the introspective school have always acknowledged that they are dealing with abstractions; let behaviorists do likewise, and we shall have no basis for quarrel and controversies. But psychology as a full-fledged science cannot limit itself to abstractions; it must deal with *concrete* facts of human nature and therefore must be an *objective* science instead of an abstract science. But this means that the most exact observations and calculations of the behaviorists never tell the complete truth about human nature and must therefore be supplemented by the introspective method. This is an entirely different reason for using introspection than the one usually advanced by behaviorists. According to their tardy admissions introspection, being only a behavioristic form, is reluctantly permitted "where the processes involved are especially subtle and intricate."⁵⁵ My contention, on the contrary, is that introspection must always be an essential part of our study of human nature, because without it we lose sight of the concrete objects of our investigations and handle only abstractions.

⁵⁵ Dashiell, *op. cit.*, p. v.

Has this somewhat rambling discussion helped us to a better understanding of the nature of psychology, so that we can arrive at a definition of this science analogous to the definitions of other sciences quoted before? If so, what is our definition of psychology, and, more specifically, what is 'objective psychology'? Here is my answer: *Psychology is one of the sciences of human nature which, from the existential and the genetic point of view, deals with the concrete ways in which human beings are impressed by, and respond to, their physical and social environment.* This is by no means a perfect definition, if it is at all possible for human beings to formulate perfect definitions, and it requires, of course, a further elucidation of certain terms used. But it is offered here as a workable definition which, on the one hand, is elastic enough to allow for growth or progress and for overlapping of subject-matter with other sciences. On the other hand, it is specific enough to set off psychology definitely from the purely biological sciences as well as the sociological sciences. Our definition avoids also such objectionable and prejudicial terms as mind, consciousness, behavior, mental life, conduct, and the like. The phrase 'from the existential and the genetic point of view' is inserted because each of the different sciences of human nature has its own specific points of view from which it looks at certain facts of human life. The subject-matter of these human sciences is therefore not so much a question of which kinds of facts are included but from which angle the facts are studied. From the existential point of view psychology therefore is mainly interested in *what kinds* of concrete, objective ways human beings are impressed and in *what kinds* of concrete, objective ways they respond.

From the genetic point of view psychology deals with the gradual changes which these impressions and responses undergo in the lifetime of an individual. Psychology is not interested in the questions whether these impressions and responses have, for example, a biological survival function or ethical or æsthetic values, or even any sociological duties of adjustment.

Nor does our definition wish to draw a sharp line between impressions and responses; for a human being to be impressed is already a response to a preceding event, as Woodworth has pointed out.²⁶ Again, any impression may be viewed also as a stimulus to a subsequent response which, in turn, will become a new stimulus to the next response, and so on, almost *ad infinitum*. The distinction between impression and response is one of convenience or emphasis upon what either comes before or what follows after; just as in physics every event is both an effect of a preceding event and also a cause of a subsequent event.

We now come to our final question: What are the objectives of this concrete or objective psychology? We have already stated that all sciences have a common, ultimate aim in view, namely the collection, systematization and interpretation of facts about our universe, and psychology shares this ultimate with its sister sciences. In psychology we aim, first of all, at a complete and accurate description of all the concrete impressions and responses of which human beings are capable. Such a description requires a careful analysis and synthesis of the facts observed and a precise statement of the essential conditions under which the observed events take place. Having thus collected a sufficient body of psychological facts, it becomes necessary to group and classify them by discovering similarities and relations to other facts. And finally, psychology aims to interpret its facts in the light of general laws or principles that can be logically deduced from them, in order to be able to predict, on the basis of such generalizations, what kinds of impressions or responses human beings will manifest under certain prescribed conditions. In these formulations of our objectives I have tried to show that psychology is pursuing in its own field of work the same objectives that all sciences are striving for in their particular realms of study, and in this I believe we shall find little, if any, disagreement among psychologists.

But it is also possible and desirable to restate our objectives more specifically in terms of our subject-matter in

²⁶ R. S. Woodworth, *Psychology, a study of mental life*, 1921, p. 187.

order to realize more fully just what in particular psychologists are aiming to do. Here, however, I am prepared to find much more disagreement among our fellow-workers, at least in verbal formulations, if not in actual understanding. Nevertheless I shall venture to propose here my own statement as it has gradually become clarified in my own mind while preparing this address, in the hope that it will help some of my listeners to clarify their own notions about the aims or objectives of psychology. And if my statement should lead to a greater harmony among a large number of psychological co-workers I should feel highly gratified for having brought this topic before you to-night.

According to my understanding of this question it is the aim of psychology to determine as accurately and fully as possible (1) what kinds of impressions human beings receive from the external world, from other human beings, and from their own internal activities; (2) how these impressions are influenced by such factors as heredity, physiological structures and functions, growth and learning, and peculiar external and internal conditions; (3) what kinds of responses human beings are capable of, how they come about, and how they are influenced by the same factors of heredity, physiological structures and functions, growth, learning, and peculiar external and internal conditions; (4) what general laws and principles seem to underlie these various facts; and (5) what applications can be made of the knowledge of these facts and laws to various fields of practical human endeavor. Here, then, is the climax of my speech, and according to all rules of rhetoric here I should stop.

But I feel that I should explain two minor points, because I have not mentioned the word measurement and I seem to leave no room in either my definition or my objectives of psychology for 'animal psychology.' To begin with the second item, I am convinced that the first concern of psychology is with human nature, and while we may be interested in the problem to what extent animals receive impressions and make responses like human beings, the converse problem properly belongs to biology. This is a question, not of

subject-matter, but of point of view, as I stated before. When the intimate relations between consciousness and behavior are more fully understood, due to greater refinements in our subjective and objective methods of observation, then we may be able to answer satisfactorily the question whether animals are conscious or not. At present we are able to make a parrot *talk* like man without understanding man, and to some extent a dog or ape *understand* man without talking like man. Is it too much to expect that some day we will be able to make an animal both talk and understand like a human being? Such a result would indeed justify greater emphasis upon animal psychology than I am at present willing to give it.

And now about measurement, the Kantian stumbling-block. From our study of the aims of other sciences we find even the most exact of them do not make measurement as such an aim in itself, but use it as a tool for standardizing their methods of observations and statements of results obtained. In a similar way, in psychology, measurement should never be looked upon as an aim in itself, as some seem to wish to do, but only as a means to an end, whenever it helps to greater accuracy of observation and clearer statement of results. But to refuse to study psychological facts because they cannot be measured is making an end out of a means and runs counter to the general objectives of all science. Psychologists should not merely aim at measurement as such, but use measurement wherever possible and helpful to a better understanding of the facts in question. Measurement may become harmful to the progress of our science if it is pursued for its own sake and thereby lead to a misconstruction of concrete facts in terms of abstract mathematical units and relationships. The force of a fruitful idea, such as Einstein's theory, can never be calculated in terms of ergs, and yet it may accomplish more than the largest engines that can be constructed. The greatest physicists themselves well recognize this fact; why should psychologists apologize for dealing with such immeasurables?

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RENÉ DESCARTES

A STUDY IN THE HISTORY OF THE THEORIES OF REFLEX ACTION¹

BY FRANKLIN FEARING

Northwestern University

It is necessary that I describe for you first the body, by itself, then the soul, and finally, that I show you how these two natures are to be joined and united to compose men resembling us.

I postulate that the body be nothing other than a statue or machine of clay which God makes expressly as near like us as possible, not alone giving it on the outside the color and forms of all our limbs, but putting inside as well, all the parts necessary for it to walk, eat, breathe, and, in fact, imitate all of our functions which can be imagined to proceed from matter, and to depend only on the arrangement of the organs.

These are the words with which Descartes in the middle of the seventeenth century introduced the first European treatise which attempted a systematic account of neuromuscular action and physiological psychology.² The principle, here formally enunciated, that the major adjustive activities of the body may be explained adequately without the introduction of metaphysical or non-mechanical 'cause,' has played an important rôle in the subsequent development of biology and psychology.

The advances in biological science during the seventeenth century were rapid and dramatic. In the previous century Vesalius had thrust aside the 1500-year-old canons of Galenic physiology and anatomy, and had substituted in their place his own observations of the structures of the human body; in 1628 Harvey had published the results of his observations of the circulation of the blood in living animals; but it

¹ Grateful acknowledgment is hereby made to the writer's colleague, Dr. E. L. Clark, for assistance with the English translations of the 'Traité de l'Homme.' Dr. Clark and the writer are collaborating on an English translation of this important treatise of Descartes'.

² *Traité de l'Homme*, 1662.

remained for Descartes to formulate a statement which described the integrative activity of the animal in terms of automatic, self-acting mechanisms.

The agencies by means of which the animal is able to move were the particular objects of biological study in the seventeenth century. The application of the principles of the 'new' physics and mechanics to the problems of muscle and nerve marks the beginnings of physiological psychology in the modern sense. The contributions of Borelli, Swammerdam, Glisson, Stenson, Mayow and Willis indicate the gradual emergence from speculative obscurity of scientific conceptions of neuromuscular action. René Descartes, more than any other man, is responsible for concepts of neuromuscular function which are acceptable in their major outlines to present-day physiologists. He had, says Brett (2), "the advantage of coming after Vesalius and being acquainted with the discovery of Harvey."

Although Descartes stands as the most important figure in the development of scientific thought since the Mediæval period, he was primarily a systematist rather than an original investigator in the field of physiology. It was with the object of confirming a psycho-physical theory of man that he made dissections and anatomical preparations. He made liberal use of the findings of others, and where attested knowledge was wanting, he did not hesitate, in the interests of his theory, to make assumptions as to structure and function.

Descartes was born in La Haye in Touraine in March, 1596.³ He was educated in the Jesuit college at La Fleche in Anjou where he spent the years 1604 to 1612. He travelled widely in his youth, visiting Paris, Italy, Hungary, Germany and Holland. He settled in the latter country in 1629 when he was thirty-three years of age. He lived in Amsterdam, Utrecht, Egmond, and Leiden, with an occasional visit to

³ The two biographies in English from which material for the present study was drawn are those of Mahaffy (10) and E. S. Haldane (8). The most important biography in French is that of Baillet (1), a contemporary of Descartes'. See also Foster (5).

Paris. In 1649, at the invitation of Christina, Queen of Sweden, he went to Stockholm where he died on February 11, 1650, at the age of fifty-four years.

Of the four important publications appearing before Descartes' death,⁴ the first ('Discourse on Method'), published when the author was forty-one years old, marks, according to Mahaffy (10), 'an epoch in the history of human thought.' The principal contributions to physiological psychology are contained in 'The Passions of the Soul,' published in 1649 just before the author's death (3), and in the 'Traité de l'Homme' (4) published in 1662. The latter is regarded by Garrison (7) as the first European text-book on physiology. It is also probably the first attempt to present systematically a coherent description of bodily responses in terms of actual—or hypothetical—neuromuscular structures.⁵ Although this work was not published until after Descartes' death, it was sketched out in 1634 together with the treatise 'On the Formation of the Foetus.'⁶ The latter was published in 1664.

Automatic Action

In order to comprehend Descartes' conception of neuromuscular coördination, it is necessary to understand his theory of bodily automatism. Essentially, this theory holds

⁴ The discourse on method (1637), Meditations (1641), Principles of philosophy (1644), and the Passions of the soul (1649).

⁵ This treatise is divided into five parts as follows: I. On the bodily machine. II. How the machine moves itself. III. On the exterior senses. IV. On the interior senses. V. On the structure of the cerebrum, how the spirits are distributed in order to cause movements and sensations.

⁶ It is interesting to note that the time when Descartes was engaged in these physiological researches coincides with the birth of his daughter, Francine (1635), the mother of whom is unknown. Mahaffy (10) has suggested that there is more than a coincidence in these events. He says: "It is not a little remarkable that this (1634) was the very year when he had peculiar opportunities of making 'observations' concerning the subject of the earliest development of man. There was born to him on the 19th of July, 1635, at Deventer, a daughter, the events of whose brief life he noted on the fly leaf of a book. 'Concepta fuit Amstelodami die Domini 15 Oct. 1634,'—that is to say, while he was specially engaged with his physiological researches. . . . Is it possible that he carried his theory of *bêtes machines* a step higher than he confessed in public, and that this adventure was merely the result of scientific curiosity?" pp. 63-64.

that all motions of animals and man are dependent on the operations of bodily structures. The body acts as a machine and its motions are explicable in terms of the laws which govern all physical machines. Descartes had been impressed by the fountains in the royal gardens which were so constructed that, actuated by water, lay figures moved, made sounds and played instruments. He conceived the animal body to be actuated on the same principle; instead of pipes and water there are nerves and animal spirits. This conception is made clear in the 'Traité de l'Homme' in the following quotation.

Now as these spirits enter thus into the cavities of the brain, so they pass thence into the pores of its substance and from these pores into the nerves. And, according as they enter or even only as they tend to enter more or less into this or that nerve, they have the power of changing the form of the muscle into which the nerve is inserted, and by this means making the limbs move. You may have seen in the grottoes and fountains which are in our royal gardens that the simple force with which the water moves in issuing from its source is sufficient to put into motion various machines, and even to set various instruments playing, or to make them pronounce words according to the varied disposition of the tubes which convey the water.

And indeed one may very well compare the nerves of the machine which I am describing with the tubes of the machines of these fountains, the muscles and tendons of the machine with the other various engines and springs which serve to move these machines, and the animal spirits, the source of which is the heart and of which the ventricles are the reservoirs, with the water which puts them in motion.⁷

Breathing and other such acts depend upon the flow of spirits in the tubes in the same way that the water causes the mill to operate continuously. The effect of an external stimulus on bodily movements is also explained in purely mechanical terms.

⁷ Quoted by Foster (5), pp. 262-3. See also the Tannery edition of Descartes' *Œuvres: Traité de l'Homme*, vol. 11, pp. 30 ff. Unless otherwise noted the Tannery edition is the one used in the translations quoted in the present paper.

External objects, which by their mere presence act upon the organs of sense of the machine and which by this means determine it to move in several different ways according as the parts of the machine's brain are disposed, may be compared to strangers, who entering into one of the grottoes containing many fountains, themselves cause, without knowing it, the movements which they witness. For in entering they necessarily tread on certain tiles or plates, which are so disposed that if they approach a bathing Diana, they cause her to hide in the rosebushes, and if they try to follow her, they cause a Neptune to come forward to meet them threatening them with his trident.

The 'Traité de l'Homme' is a statement of how the human body *might* carry on its functions if it were a machine constructed according to known laws of mechanics. In the 'Discourse on Method,'⁸ Descartes was careful to note that even though such automata might be constructed which would in all respects simulate the actions and appearances of men, yet it would be possible by the application of 'two very certain tests' to determine whether or not they were real men. In the first place such automata could never use speech appropriately in order to reply to anything that might be said to them. In the second place, inasmuch as these machines do not act from knowledge but from 'the disposition of their organs,' they could act only in those situations for which they were prepared; that is, it would be 'morally impossible that there should be sufficient diversity in any machine to allow it to act in all events of life in the same way as our reason causes us to act.' By the application of these two tests it is possible to distinguish between men who possess souls infused in them by the Deity, and brutes which are regarded as clock-like automata.

In a letter to the Marquis of Newcastle (11), Descartes says:

I know, indeed, that brutes do many things better than we do, but I am not surprised at it; for that, also, goes to prove that they act by force of nature and by springs, like a clock, which tells better what the hour is than our judgment can inform us. And,

⁸ Haldane translation (8), vol. I., pp. 116 *f.*

doubtless when swallows come in the spring, they act in that like clocks. All that honey-bees do is of the same nature; and the order that cranes keep in flying, or monkeys drawn up for battle, if it be true that they observe any order, and finally, the instinct of burying their dead is no more surprising than that of dogs and cats, which scratch the ground to bury their excrements, although they almost never do bury them, which shows that they do it by instinct only, and not by thought. . . . If they [the brutes] should think as we do, they would have an immortal soul as well as we, which is not likely.

The soul in man only acts through the bodily mechanisms, that is, the mind can only act indirectly on the body. The soul is located in the 'little gland'—the pineal gland—in the center of the brain. At this point the mind, by special arrangement of the Deity, is in contact with the nervous system. The structure of the nerves is described in the following terms ('*Traité de l'Homme*'):

Notice that in each of these little tubes [the nerves] there is something like a marrow composed of several very thin threads [*filet*] which come from the very substance of the brain, and which terminate at one end on the inner side of the brain cavity and at the other end in the skin and flesh where the tubes containing them terminate. But, because this marrow does not serve at all in the movement of members, it is sufficient, for the present, that you know that it does not fill so much the tubes which contain it, as do the animal spirits which yet find here room enough to flow easily from the brain to the muscles, where these little tubes, which here must be considered as so many little nerves, are distributed.

Sensory action is explained by the action of these 'delicate threads' which compose the marrow of the nerves. These threads are attached to the sensory organs at one end and to the 'orifices of certain pores which exist on the internal surface of the brain' at the other. When the sensory organs are excited by external stimuli they cause a slight pull on these threads which opens the orifices in the brain and permits the animal spirits to flow towards the muscles. These processes were described by Descartes as follows:

In order to understand next how this machine can be incited by the external objects which strike upon its organs of sense to move in a thousand different ways all its members, remember that the little threads, of which I have already spoken so much, coming from the innermost part of the brain and composing the marrow of its nerve, are so placed in all of its parts which serve as sense-organs that they can very easily be moved by the objects of its senses. And remember that whenever they are moved, no matter how little, they pull at the same instant the parts of the brain from whence they come, in this way opening the entrances to certain pores, which are on the internal surface of the brain. Through these pores the animal spirits, which are in the cavities, immediately take their course through the nerves to the muscles which serve to make movements in this machine very like unto those we are induced to make when our senses have been touched in the same way.

It is interesting to note that Descartes did not distinguish clearly between sensory and motor nerves, but believed that the nerve tubes both contained the 'delicate threads' essential to sensory action and served as a canal through which the animal spirits were conducted to the muscles.

Motor action consisted in the inflation of the muscle by the animal spirits which were conducted to it by the nerve. This is made clear in the following passage:

For you know very well that these animal spirits, being like a wind or very fine flame, cannot fail to flow very quickly from one muscle to another as soon as they find a passageway; although there is no other power to move them excepting only their inclination to continue their movement, according to the laws of nature. And you know, besides this, that as long as they remain very mobile and fine, they do not lose the force to inflate and make rigid the muscles in which they are enclosed: this happens just as the air in a balloon hardens it and stretches the skins which contain it.

The problem of simultaneous relaxation and contraction of antagonistic muscles, *e.g.*, the coördination of the internal and external recti which turn the eyeball, was explained by assuming the existence of valved channels between the opposing muscles through which the animal spirits were

conducted from one to the other. These valves were so arranged that the inflation of one muscle made impossible the inflation of the other, and at the same time permitted the spirits of one muscle to flow into the other.

In the 'Traité de l'Homme' Descartes has reproduced a kneeling human figure near a fire. In this drawing he has diagrammed the assumed nervous channel from the foot to the brain and described the neuromuscular events which occur when the foot is withdrawn from the fire.

If, for example, fire comes near the foot, the minute particles of this fire which, as you know, move with great velocity, have the power to set in motion the spot of the skin of the foot which they touch, and by this means pulling upon the delicate thread which is attached to the spot of the skin, they open up at the same instant the pore against which the delicate thread ends, just as by pulling at one end of a rope one causes to strike at the same instant a bell which hangs on the other end. When this pore is opened the animal spirits of the cavity enter into the tube and are carried by it partly to the muscles which pull back the foot from the fire, partly to those which turn the eyes and the head in order to regard it, and partly to those which serve to advance the hands and to bend the whole body in order to shield itself.

Thus we have a more or less complete account of an integrated sensori-motor action expressed in purely mechanical terms and unaccompanied by consciousness. Consciousness appeared when a beneficent Deity equipped man with a soul which could modify bodily action only through the agency of the animal spirits.

The response of the neuromuscular system to stimulation is dependent upon 'six different sorts of circumstances,' as follows: (1) The particular sense organ which, by means of the small threads, releases the animal spirits in the brain. (2) The intensity or force of this releasing action. (3) The arrangement of the little threads which compose the substance of the brain, that is, the arrangement may be either 'natural' (native) or acquired. (4) The unequal strength possessed by the different parts of the spirits which may be a factor in determining their course. (5) The diverse situation of the

exterior members. (6) The organization by means of which coördinated action is possible, *e.g.*, walking.⁹

In the above list the third item is of particular interest, since it suggests a distinction which is of significance to modern psychology. By the 'innate' arrangement of fibers in the brain, Descartes meant that 'God had so disposed' them that the animal spirits are sent 'toward all the nerves whither they should go in order to cause the same movements in this machine towards which a similar action could excite us following the instincts of our nature.'

Animal Spirits

The animal spirits, according to Descartes, were a very powerful but subtle fluid, wholly unique, but subject to physical law. Its function was to link the soul with the body, and to inflate the muscles to which it was carried by the nerves. This fluid was distilled from the blood and stored in the brain.

The process by means of which the food is converted into blood and eventually into animal spirits occurs in the stomach, liver and brain. It consists essentially in breaking up the food into finer and finer particles. In the heart the blood is expanded and dilated by the 'fire without light' which this organ was supposed to contain. From here the "most active, strongest and finest parts of the blood go to the cavities of the brain," at which point it is converted into animal spirits. This refining process was described as follows:

As regards the particles of the blood which reach the brain, they serve not alone to nourish and support its substance, but principally to produce there a certain very quick wind, or rather a very active and pure fire that has been named animal spirits. Now it is necessary to know that the arteries, which carry them from the heart, after being divided into an infinite number of small branches and after composing these little tissues, which are stretched out like a tapestry at the bottom of the cavities of the brain, are assembled around a certain little gland, situated approxi-

⁹ *Traité de l'Homme* (Tannery edition of the *Œuvres*, vol. II, pp. 190 *f.*).

mately in the middle of the brain substance, at the entrance of these cavities. The arteries have in this place a large number of little holes through which the finest particles of the blood can flow to this gland but which are so narrow that they do not allow the larger particles to pass. . . . Thus it is easy to see that, as the largest particles go straight up to the external surface of the brain to serve as nourishment, they are the cause for the smallest and most active particles of the blood to turn aside and enter that gland which may be imagined as a very abundant fountain from which these particles simultaneously flow from all sides into the cavity of the brain. And thus without other preparation or change, except that they are separated from the largest particles and that they still retain that extreme swiftness of motion which the heart has given them, they cease to have the form of blood and are called animal spirits.

In addition to an anatomy the details of which were largely imaginary, Descartes committed himself to two major errors in his analyses of neuromuscular function: his 'animal spirits' hypothesis, and his conception that the muscle increased in bulk, *i.e.*, was inflated by the animal spirits, during contraction. He seems to have been in ignorance of the work of his contemporary, the Dutch naturalist, Jan Swammerdam,¹⁰ who attacked the problems of neuromuscular action experimentally, and was able to demonstrate the inadequacy of both the animal spirits and the muscle-inflation hypotheses.

Reflex Action

Descartes is usually credited with making the first descriptive statement of involuntary action which bears a recognizable resemblance to the modern concept of reflex action. Such a concept is implicit, of course, in his hypothesis of the automatism of brutes. The actual use of the word reflex in connection with the action of the nervous system occurs in the following passage.

For in certain persons that [previous associations] disposes the brain in such a way that the spirits reflected from the image thus

¹⁰ See Fearing (6) for a discussion of Swammerdam's contributions to the physiological psychology of the 17th century.

formed in the gland [of an approaching animal] proceed thence to take their places partly in the nerves which serve to turn the back and dispose the legs for flight, and partly in those which so increase or diminish the orifices of the heart, or at least which so agitate the other parts from whence the blood is sent to it, that this blood being there rarefied in a different manner from usual, sends to the brain the spirits which are adapted for the maintenance and strengthening of the passion of fear, *i.e.*, which are adapted to the holding open, or at least reopening, of the pores of the brain which conduct them into the same nerves.¹¹

The following excerpt from 'Objections and Replies'¹² (Reply to Objection IV.) makes clear the mechanical nature of this type of action.

But the greater part of our motions do not depend on the mind at all. Such are the beating of the heart, the digestion of our food, nutrition, respiration when we are asleep, and even walking, singing and similar acts when we are awake, if performed without the mind attending to them. When a man in falling thrusts out his hand to save his head he does that without his reason counselling him so to act, but merely because the sight of the impending fall penetrating to his brain, drives the animal spirits into the nerves in the manner necessary for this motion, and for the producing it without the mind's desiring it, and as though it were the working of a machine.

The same idea is found again in the following ('The Passions of the Soul,' Article XIII.).

To follow this example it is easy to conceive how sounds, scents, tastes, heat, pain, hunger, thirst and generally speaking all objects of our other external senses as well as of our internal appetites, also excite some movements in our nerves which by their means pass to the brain; and in addition to the fact that these diverse movements of the brain cause diverse perceptions to become evident to our soul, they can also without it cause the spirits to take their course towards certain muscles rather than towards others, and thus to move our limbs, which I shall prove here by one example only. If someone quickly thrusts his hand against our eyes as if to strike us, even though we know him to be

¹¹ Passions of the soul (3), Article XXXVI.

¹² See Haldane translation (3).

our friend, that he only does it in fun, and that he will take great care not to hurt us, we have all the same trouble in preventing ourselves from closing them; and this shows that it is not by the intervention of our soul that they close, seeing that it is against our will, which is its only, or at least its principal activity; but it is because the machine of our body is so formed that the movement of this hand towards our eyes excites another movement in our brain, which conducts the animal spirits into the muscles which cause the eyelids to close.

The reflex type of response cannot be directly controlled by the will, according to Descartes, although it may be indirectly modified by the action of the soul. The following passage from the 'Passions of the Soul' is quite remarkable in its agreement with modern interpretations. Descartes is discussing the dilation of the pupil with far fixation.

At the same time it is not always the desire [volition] to excite in us some movement, or bring about some result which is able so to excite it, for this changes according as nature or custom have diversely united each movement of the gland to each particular thought. Thus, for example, if we wish to adjust our eyes so that they may look at an object very far off, this desire causes their pupils to enlarge; and if we wish to set them to look at an object very near, this desire causes them to contract; but if we think only of enlarging the pupil of the eye we may have the desire indeed, but we cannot for all that enlarge it, because nature has not joined the movement of the gland which serves to thrust forth the spirits towards the optic nerve, in the manner requisite for enlarging or diminishing the pupil, with the desire to enlarge or diminish it, but only with that of looking at objects which are far away or near. (Article XLIV.)

Descartes was concerned primarily with the analogy between mechanical and physiological action. Brett (2) observes that Descartes saw only the points of resemblance between the reflection of light, reflux of water and reflex action, while the modern neurologist would see chiefly the absence of such resemblance.¹ Included under the Cartesian concept were such activities as breathing, singing, walking, swallowing, yawning, bodily accompaniments of emotion,

¹ It should be noted also that for Descartes the reflex arc involved brain rather than spinal structures.

eye movements, intra-ocular adjustments, excretory actions, protective responses to external stimuli, postural responses, etc.—actions which involve a considerable degree of integrative complexity.

Huxley, who regarded Descartes as a physiologist of the first rank, summarizes (9) the latter's contributions to the physiology of the nervous system as follows: (1) The brain was established as the organ of sensation, thought and emotion. (2) Muscular motion is due to change in the form of the muscles which in turn is due to motion of the substance contained in the nerve which goes to the muscle. (3) Sensation is due to change in the substance of the nerves which connect the sense organs with the brain. (4) Reflection of motion from a sensory into a motor nerve may take place without volition or even contrary to it. (5) Motion of the matter in the brain occasioned by the sensory nerve leaves behind a disposition to be moved again in the same way.¹³ While we may not, perhaps, be able to echo Huxley's enthusiastic estimate of Descartes as a physiologist, the significance of the foregoing list of contributions cannot be denied. They had a profound influence upon the attempts of those who followed after Descartes to deal with the problems of animal adjustment. Not all of these findings are original with Descartes, but he is responsible for their expression in lucid and systematic form.

With the example of Galileo before him, Descartes had every reason to be extremely cautious in dealing with those topics on which the Mother Church might be expected to be sensitive. He described how the animal body might behave if it were a machine acting in accordance with the known laws of physics and mechanics. In man he retained the soul which, through the mediation of the animal spirits, could influence and be influenced by the body. He observed that many of the adjustmental activities of the human body could be carried on independently of the soul.

The recognition of the importance of these automatic activities and the principle of reflex action, together with

¹³ See the 'Passions of the soul,' Article XLII.

the concept of animal action as dependent upon and explainable in terms of the laws of physical mechanisms, constitute Descartes' major contributions to physiological psychology.

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INHIBITION AND LEARNING

BY A. L. WINSOR

Department of Rural Education, Cornell University

In developing positive and negative conditioned salivary responses with human subjects, fundamental aspects of adaptive behavior have been observed which if taken into consideration may account for some of the inadequacies in current theories of learning. A confessed limitation of these theories, particularly the law of exercise, is the inability of the principle, as stated, to account for the elimination of wrong responses or undesirable habits. Adaptive behavior is frequently cited whereby repetitive stimulation causes a decreased response, and the proponents of the law of use speak of such occurrences as limitations (1) of the principle.

The fact that research has been largely limited to positive adjustment accounts in part for the greater vulnerability of theories of learning relative to negative adjustment. Due to the simple appearance and early recognition of the direct relationship of increased response to increased stimulus much work has been done on this problem. Negative learning, on the other hand, by which increased stimulation arouses decreased action has appeared strange and contrary to both biological and physical phenomena. As a result inhibition as an adaptive process has either been ignored or misconstrued. If a blind path or other ineffective response in the learning situation was to be eliminated the procedure suggested has been either to substitute another stimulus, or rely on forgetting to effect the change. It seems rarely to have been assumed that nature should provide an active process for the elimination of false signals. Yet the fact that negative adjustment is just as common and just as significant as positive adjustment seems too obvious for discussion. It is apparent, furthermore, that the substitution of a new

stimulus for the inadequate one is as impracticable as a corrective measure as the alternative suggestion of disuse. When a child has developed a fear response to the sight of dogs, the substitution of cats or some other stimulus in place of the exciting agent would be as hopeless for practical purposes as an attempt to prevent contact with dogs. Yet children overcome the fear of dogs just as wild horses overcome the fear of man, by repetition of the stimulus. It is a description of this process together with its implications that we are attempting in this report.

The conditioned reflex technique of Pavlov (2) affords opportunity for a quantitative description of the process of extinction of a previously active response. By measuring the parotid secretion for successive periods under constant unreinforced stimulation, the change from a positive to a negative response may be shown. The salivary response affords particular advantages for this type of study because of the necessity of a quick adjustment of such reactions to significant changes in the environment. Under the name of 'experimental extinction' many examples of this type of adaptation in the behavior of dogs have been shown by Pavlov. During the past year we have been successful at Cornell in adapting and applying this technique to man, and data from these experiments will be used as a basis for our discussion. Since the factors affecting parotid secretion (3) and the procedure used (4) have already been reported it will be possible to begin at once with an analysis of data. It should be emphasized, however, that the secretion of these glands to a food situation is an acquired response, notwithstanding frequent statements to the contrary. A natural stimulus was used in place of an artificial conditioned stimulus because it represents a firmly established response, and it avoids the cry of artificiality sometimes raised against conditioned reflex data. The subject was an adult male who was in a state of hunger for each test and had been eating his luncheon under the conditions of the experiment for a protracted period.

The control reaction in Fig. 1 shows the amount of saliva

being secreted when the subject was seated quietly without food as a part of the situation. In the next minute food was placed on the table and a 300 per cent increase in saliva is

EXPERIMENTAL EXTINCTION

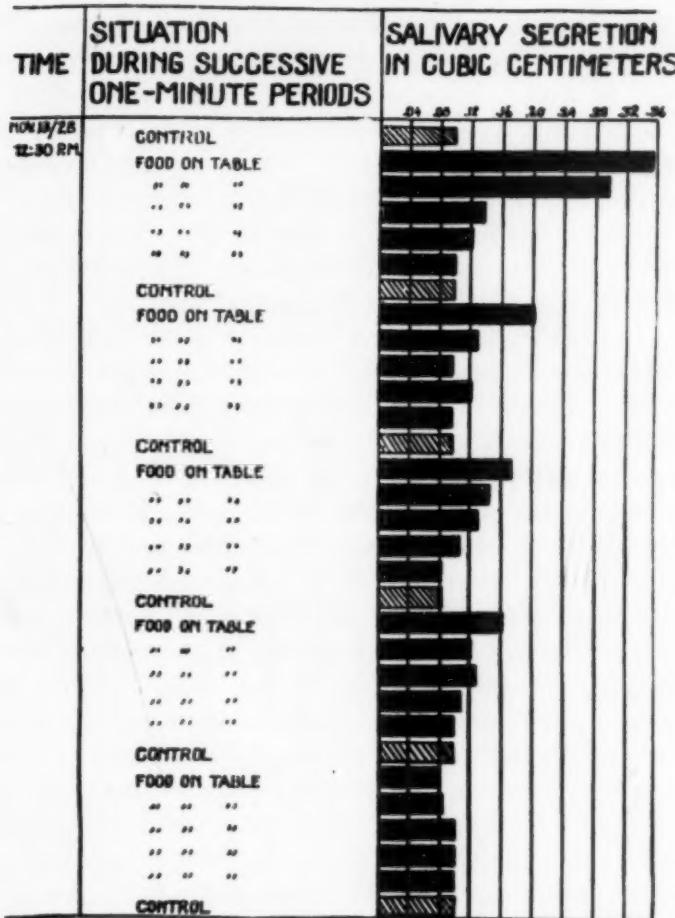


FIG. 1. Column diagram showing the increase in the quantity of saliva secreted when food is placed before the subject and the development of inhibition when the food stimulus is not reinforced.

registered. The quantity obtained for successive minutes of continued stimulation thereafter without reinforcement (without eating) shows a progressive decrease. In other words,

there appears relatively soon a transition into a state of inhibition of the central processes acted upon by the conditioned stimulus. This transition is accompanied by a gradual increase in the latent period of the reaction although that is not shown in the figure. The food, now no longer eaten when seen, loses its biological significance as a signaling agent and the organism quickly adjusts itself to the new state of affairs.

During the second control period all food was removed from sight for one minute and when it was replaced in the next period the response was partially restored. The rest period served to release the inhibition that was developing by continued unreinforced stimulation. This phenomenon Pavlov calls dis-inhibition or a temporary removal of inhibition. If we are to speak of 'dis-inhibition,' consistency demands that we speak also of dis-excitation. This instability in the early stages of the development of inhibition is not unlike the instability previously reported (4) in the early stages of the excitatory process. It was shown earlier that when a conditioned response is developing the introduction of a time interval between repetitions of the stimulus affected the strength of the response. Similarly when an inhibitory response is developing the introduction of a time interval between repetitions of the un-reinforced stimulus affects the strength of the inhibitory response. Both processes are readily disturbed in the early stages of their fixation process. Such a term as interference which could be applied to both aspects of adjustment would seem to be better. Continued stimulation by the food caused a reinstatement of the inhibitory state with the characteristic release following each rest period until inhibition became firmly established. A stimulus to a positive conditioned response has under the conditions of this experiment been transformed into a stimulus for a negative or inhibitory conditioned response. This transition was seen to be fairly rapid and progressive. The positive response or habit was eliminated not by discontinuing the stimulation (disuse) or by substitution of another stimulus. Neither does such a phenomenon represent a limitation of

the law of use. It was accomplished by the continuation of a stimulus whose biological significance to the organism had changed.

The question may arise relative to the designation of this process as inhibition as against permanent destruction or fatigue of the response. The fact that the extinguished response spontaneously regenerates itself in course of time discounts permanent destruction as an explanation. Cortical fatigue seems to be ruled out by the fact that the secretory elements do not become fatigued when the conditioned responses are being reinforced (5). Moreover further secretory activity of the gland by an immediate application of the food is possible, showing that neither the glands nor the nervous centers of the secretory response are fatigued. If further evidence is necessary to show that fatigue does not account for this reduction of the response, a conclusive fact is the spread of inhibition to centers that have not been recently active, causing inhibition of these centers as shown by Pavlov. The question might well be raised if what has been termed fatigue when sense organs failed to respond after continuous stimulation for a few minutes is not inhibition rather than fatigue.

Immediately after performing the test reported above the subject began eating again under the same conditions as before. Since he was looking at the food as he ate the response to the sight of food underwent another transformation and in a short time became positive once more. Unlike the glands of the stomach the salivary glands must adjust very quickly because of their function.

After four days the response to the sight of food was again extinguished under the conditions shown in Fig. 2. Here as before the first minute period shows a normal or control response. In the next period food was placed on the table and the secretion was materially greater. The food was left on the table for the next three periods without any of it being eaten and the organism began the process of developing an inhibitory response to the food situation. In the sixth period a metronome set at 120 beats per minute

was released near the subject. The new stimulus now became a disturbing agent which like the rest period in Fig. 1 dis-inhibited the inhibition and made it necessary for the organism to repeat the process of fixation of inhibition. The release takes place after a long latent period, causing the greatest response in the second period after the stimulation. Many other factors affect the progress of inhibitory activity but the scope of this paper precludes their consideration at

DIS-INHIBITION

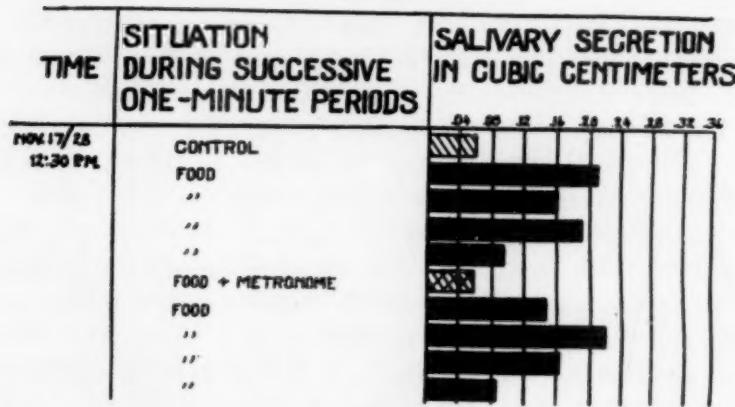


FIG. 2. Column diagram showing dis-inhibition when a disturbing stimulus occurs during the fixation of the inhibitory response.

this time. In general it may be said that the same temporal and spatial stimulation factors as well as the same organic influences known to affect positive adjustment affect negative adjustment. We also have some evidence that the effect of such a drug as caffeine disturbs the orderly development of inhibition. These data will be reported later.

If we may assume that the increased secretion of the parotid to the sight of food represents a typical organic habit and that under the conditions of the experiment it was 'broken' or eliminated, these data become very significant to students of behavior. They afford a study of the factors which cause a positive or excitatory reaction to be changed into a negative or inhibitory reaction. The factor of exercise is rather clearly shown. If any one aspect of the total

situation or organic influence could be singled out as of especial potency in the fixation of inhibition it would be repetition. Other factors remaining constant, this factor of exercise is seen to facilitate inhibition as well as excitation. An important factor in the breaking of this organic secretory habit, therefore, is use. In order to conserve energy nature is equipped to eliminate false signals as well as to utilize adequate signals. Both processes appear to be subject to the same important basic laws. Whether the reaction develops into a positive or negative habit depends upon the biological significance of the situation. As the significance of the signal to the organism changes, persistence of that signal causes a change in the response.

An examination of almost any psychology text which describes the learning process will show that this dual aspect of adaptation has not been taken into consideration. It appears from the texts that the authors conceive of repetitive stimulation as inevitably increasing the probability that the response will continue to be positive. It seems never to have occurred to them that exercise might function in developing a negative response. In fact they try to account for adaptation without this type of a response and when they are confronted with inhibitive behavior assume that it is merely a limitation of the law of exercise.

To be specific we shall quote from the discussion of the learning process by Gates (1). The quotation will illustrate the process of experimental extinction and at the same time show the dilemma writers find themselves in without such a concept.

Under the general heading 'The Elimination of Connections by Simultaneous Association' (p. 290) Gates quotes this experiment performed by G. W. and E. A. Peckham.

These observers found that a spider dropped hurriedly from its web at the sound of a tuning fork. When it had climbed back a repetition of the stimulus produced the same dropping reaction; but after eight or nine trials the stimulus suddenly lost its power; the spider failed to react by dropping from the web. Next day, however, the stimulus was effective for a time but failed after six

or seven repetitions, and after about ten days the dropping reaction ceased entirely—at least for a time.

This is really a most curious matter (explains Gates). The dropping to the ground reaction has been eliminated in the very process of exercise. According to the law of use, the tendency to drop at the sound of a tuning fork should have been strengthened; should have become more prompt and certain. Instead of that it gradually became less prompt and certain, finally being eliminated entirely.

This phenomenon might well bring the law of use under serious suspicion. This experiment, however, does not really discredit the law but portrays one of its limitations.

The behavior of the spider illustrates the transition of a positive response into a state of inhibition and shows an important application of the law of use rather than a limitation of it. Numerous examples of such behavior, which the authors have assumed to be opposed to the law of exercise, could be cited from other texts. We shall content ourselves with but one more such example. Jordan (6) (p. 72), cites this case.

"An individual living near a railroad may at first hear every train, but in course of time repetition or exercise seems to *weaken* the very process of learning so that the sound may be neglected."

Because the response is inhibited the process of learning is assumed to be weakened. Obviously no weakening in adjustment has taken place although a reduced response appeared. The organism has made progress and is more efficient with the negative response to the railroad noises established. Learning or adaptation has proceeded and has not been weakened.

Two experimental studies have recently appeared in scientific journals which will show the possible implications of this concept in practice. Dunlap (7) 'broke' the habit of writing 'hte' for 'the' on the typewriter by practicing 'hte,' knowing it was incorrect. He postulated the hypothesis that the response in itself has no effect on the future probability of the same stimulus pattern producing the same

response. He called this the 'Beta' hypothesis and apparently as a result of his report Holsopple and Vanouse (8) have recently tested the hypothesis. Their procedure was to take a list of words that were habitually misspelled by students in typewriting and divide each student's mistakes into two equal groups. The students were then given their two groups to practice. They were told that these errors had been repeatedly made in their work. One group was to be repeatedly written incorrectly, as they had been writing it, for a definite number of times. The other group of words was to be written correctly the same number of times. Care was taken to see that the instructions were carried out. Dictation was then given which contained the misspelled words of both groups many times and a check was made to see which group had been learned best. All except one of the eleven students made errors in words which were practiced as correctly spelled and not a single one of the eleven students made an error in writing the words that were incorrectly spelled.

Such experiments support the concept of internal inhibition or negative learning described above and serve to remind one of similar examples. The old Keeley cure prescribed as a remedy for men addicted to the liquor habit consisted of forcing the victims to continue to drink liquor after it had become obnoxious to them. The long life of those institutions is evidence enough of the temporary success of the method. 'Wolf, wolf' when repeatedly used in play lost its significance as a distress signal. Whatever one may think of the doctrine, it is the concept that conditioned reflex research implies. In fact, it is its most significant contribution. The development of the positive conditioned reflex suggests nothing except the novelty of the technique employed, but the processes of experimental extinction offer a challenge that cannot be ignored by those who would espouse conditioning.

If now we examine the popular laws of learning in the light of this concept, the need for a re-statement becomes apparent. The laws of disuse, use, and effect are defined by Gates (p. 281) as follows:

Law of disuse: "When a modifiable connection between a situation and a response is not exercised during a length of time, the strength of the connection is decreased." If one learns by reacting or if learning takes place only during activity, as stated by this author (p. 281), it is difficult to justify the law of disuse as stated above as a law of learning. Forgetting is neither an active process nor an adaptive process. It is passive. By the definition of learning disuse is excluded as one of its aspects. The method of stating the converse of one law and calling it another law seems peculiar to psychology. If physicists were to adopt it, for example, there would be two laws of gravitation rather than one.

Law of use: "Whenever a modifiable connection between a situation and a response is exercised, other things being equal, the strength of that connection is increased." If 'the strength of the connection' is determined by the volume of the response, a glance back at the tables will show that this statement is not always true. The positive response grew less with continued stimulation although the situation was as constant as it was possible to make it. The significance of the situation to the organism, however, had changed. Somewhere in the central nervous system exercise became functional in adapting the organism to the new situation by developing an inhibitory or negative response. The nature of that process is a problem for physiological research. It is probably the ascendancy of a new factor rather than a weakening of the old bond. So far as adaptation is concerned each successive stimulation must strengthen the process or the organism would soon perish. The direction of the response may be either positive or negative, but exercise is essential in either case. With such a concept discussions of the limitations of the law would be replaced by suggestions for eliminating useless responses through the law of use.

Law of effect: "The individual tends to repeat and learn quickly those reactions which are accompanied or followed by a satisfying state of affairs. The individual tends not to repeat or learn quickly those reactions which are accompanied

by an annoying state of affairs." By this definition finding out that a fresh olive was distasteful with one bite would not be learning quickly; since "the individual tends not to learn quickly those reactions which are accompanied by an annoying state of affairs." Or if a child puts its finger in an electric light socket and is annoyed, he could not learn quickly that such a place is not especially desirable for fingers. If he is to learn quickly, by the above definition, only reward and not punishment is conducive to adjustment.

Obviously the organism adjusts itself just as quickly to an annoying state of affairs as it does to a satisfying state of affairs so far as the temporal factor alone effects it. Both types of response are constructive and integrative so far as the organism is concerned. The effect determines merely whether an excitatory or an inhibitory reaction is to be developed and factors such as exercise and intensity carry on the fixation process the same in one type as the other. Thorndike (9) (p. 71) made no reference to the speed of the adjustment in his original statement of the law. It was as follows: "To the situation, a modifiable connection being made by him between an S and an R and being accompanied or followed by a satisfying state of affairs, man responds, other things being equal, by an increase in the strength of the connection. To a connection similar, save that an annoying state of affairs goes with or follows it, man responds, other things being equal, by a decrease in the strength of the connection."

While this statement is more in keeping with the facts than the one quoted above, a more descriptive and exact statement is possible. If conditioning may be considered as representing at least one type of adaptive behavior, the factors influencing this process represent fundamental principles affecting adjustment or learning and may be stated as follows:

When an S-R activity occurs repeatedly just prior to or concurrent with an excitatory or positive S-R activity of greater potential value, the S of the former activity comes to assume some of the stimulation properties of the S of greater potential.

A substitute or conditioned stimulus is thus established. If now the conditioned stimulus occurs repeatedly without being accompanied or reinforced by the S of greater potential a negative or inhibitory response develops. For example, when bread is seen repeatedly while the salivary glands are active, the sight of bread becomes a positive conditioned stimulus for salivation. If, however, bread is seen repeatedly while the salivary glands are no longer active an inhibitory response develops and the sight of bread becomes a negative conditioned stimulus. The new stimulus seems, therefore, to acquire its stimulation properties as a result of contiguity, irradiation of excitation from its point of initiation, and induction of excitation to a point of greater potential in the central nervous system. The new stimulus may be an elementary aspect of the environment or it may be a compound stimulus forming a Gestalt or perceptual situation. Whatever the nature of the stimulus or the type of the response (positive or negative) the establishment of the connection between the two appears to proceed in accordance with the same fundamental factors. These factors may be classified tentatively as follows.

I. Organic factors:

1. Neural organization and structure intelligence
stability
experience
2. Organic states fatigue
emotion
set
3. Maturity
4. Differences in potential of innate responses

II. Stimulation factors:

1. Temporal relationships frequency
recency
duration
sequence
2. Spatial relationships intensity
summation
interference

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BEATS AND RELATED PHENOMENA RESULTING FROM THE SIMULTANEOUS SOUNDING OF TWO TONES—I

BY ERNEST GLEN WEVER

Princeton University

In our endeavor toward an understanding of the auditory mechanism we are led to consider its operation under various conditions of stimulation, and one of the simplest of these is the simultaneous sounding of two tones. Yet we are faced at once, under such conditions of stimulation, by psychological problems far from simple. For, neglecting the problems of fusion and of consonance and dissonance, we have three types of phenomena to be dealt with, namely, beats, combination-tones, and masking. If the two tones differ but little in frequency, and their intensities are somewhere near equal, we hear the peculiar surgings of sound known as beats, which become increasingly rapid as the tones are made to differ more in frequency. When the frequency-difference becomes sufficiently great, with fairly loud tones of nearly the same intensity, we hear a third tone, called a combination-tone, beside the two primaries. But if the intensity of one of the tones is considerably greater than that of the other, a third phenomenon enters, that of masking, and we hear only the stronger tone. Of these three phenomena the first, the phenomenon of beats, will be the primary consideration of this paper.

EARLY HISTORY OF BEAT PHENOMENA

Our knowledge concerning the phenomenon of beats has grown up largely in the field of musical harmony. Of the earliest observations here we have no record. Very likely beats were known to the early musicians. The organ-tuners must have been quite familiar with them, for the mistuning

of sustained tones of such strength as the organ delivers could hardly fail to bring them to notice. But for the earliest known mention of the phenomenon we must look to Marin Mersenne, who in his 'Harmonie Universelle,' in 1636, made note of the discovery that two organ-pipes not in strict unison produce a 'trembling' when sounded together. He said:

Il est certain que deux tuyaux qui sonnent en mesme temps se font trembler, lors qu'ils ne font les Consonances justes, car si l'on tient deux, ou plusieurs tuyaux qui soient tant soit peu esloignez de l'unisson, ils font trembler les mains qui les tiennent, & lors qu'ils font parfaitement l'unisson, ils ne tremblent plus.¹

The next mention of beats was by William Holder, in 1694, and is a little more descriptive of the experience, however poetical in its flavor. He wrote as follows:

It hath been a common Practice to imitate a Tabour and Pipe upon an Organ. Sound together two discording Keys (the base Keys will shew it best, because their Vibrations are slower) let them, for Example, be Gamut with Gamut sharp, or F Faut sharp, or all three together. Though these of themselves should be exceeding smooth and well voyced Pipes; yet, when struck together, there will be such a Battel in the Ayr between their disproportioned Motions, such a Clatter and Thumping, that it will be like the beating of a Drum, while a Jigg is played to it with the other hand. If you cease this, and sound a full Close of Concords, it will appear surprizingly smooth and sweet, which shews how Discords well placed, set off Concords in Composition.²

This passage is interesting in that it implies a theory of consonance and dissonance that has since become familiar to us through the championing of Helmholtz. But to Joseph Sauveur must be accredited the first formulation of the theory. In a report before the Royal Academy in Paris in 1700 he not only demonstrated the phenomenon of beats, but he stated clearly the notion that dissonance is caused by the presence of beats, and consonance by their absence.³

¹ Marin Mersenne, *Harmonie universelle*, V., *Traité des instrumens*, 1636, p. 362.

² William Holder, *Treatise of the natural grounds, and principles of harmony*; the quotation is from the 2d ed., dated 1701, p. 44.

³ Joseph Sauveur, *Sur la détermination d'un son fixe*, *Histoire de l'académie Royale des Sciences*, Paris, 1700, 134 f., 139 f.

Robert Smith's 'Harmonics,' 1749, marks a turning-point in the history of the subject of beats, for in it we have a contribution of the first importance; and this in spite of the peculiar style in which it was written, a style that led one of Smith's admirers to remark that "his book is not only the most obscure and repulsive in its own subject, but it would be difficult to match it in any subject."⁴ Yet in developing in a thoroughgoing manner the mathematical theory of recurrent patterns he indelibly impressed his influence upon the subsequent development of the subject; and though he was not often understood, he was much discussed.

During the next century the writers on natural philosophy usually paid some attention to the subject of beats, and their discussions show that they were acquainted with the simple phenomena, and with the chief problems presented. Thomas Young treated the subject in some of his physical papers, and in his 'Course of Lectures' in 1807.⁵ In the 'Lectures' he stated clearly the theory of consonance and dissonance to which Sauveur must be accorded priority; it is probable that Helmholtz got the theory from Young. Robison, a little later, discussed beats in connection with the subject of temperament in music, and included an estimation of Smith's work.⁶ But the best discussion of Smith's contributions, and of the early development of the subject, is to be found in a short article by De Morgan (1857).⁷

During this early period there was but sluggish progress. But with Helmholtz's mighty work in 1863 came the awakening, and from then on, during the remainder of the century, appeared contributions in great numbers and of the first magnitude. Koenig, Bosanquet, Stumpf, and a number of others added their names to the history of the subject.

⁴ A. De Morgan, in *Trans. Cambridge Phil. Soc.*, 1864, 10, p. 129. This reference was to the 2d ed., of 1759, which purported to be 'much improved and augmented.' See Smith's work, pp. 56-122 (of the 2d ed.), for the treatment of beats.

⁵ T. Young, A course of lectures on natural philosophy, I, pp. 390 f. The articles referred to are brought together in *Miscellaneous works of Thomas Young*, ed. by G. Peacock, 1855, I, pp. 83 f., 93 f., 131 f.

⁶ John Robison, *System of mechanical philosophy*, 1822, IV., 408 f.

⁷ De Morgan, *op. cit.*, 129 f. See also W. Pole, *Beats in music*, *Nature*, 1876, 13, 212-214, 232-234.

With them and their work we shall become familiar as the paper progresses.

THE NATURE OF THE EXPERIENCE

Two tones exactly in unison, when conducted simultaneously to the ear, will give a tone of the common frequency, with a loudness that may be anywhere from somewhat above that of either component down to complete silence, depending upon the relations of phase. If the two tones are exactly in phase, the result is a tone of maximum loudness; if they are in opposite phase, the result is silence. If one of the tones be changed very slightly in frequency, then the phase-relations will change slowly, and the resultant tone will be found to alternate between a maximum loudness and silence, at a rate that is equal to the difference in frequency of the components.

The rise and fall of the loudness of the tone is prominent in the experience so long as the rate is slow, but when with an increase in the frequency-difference of the components the beats become more rapid, this is no longer the case. We then hear merely pulses of tone separated by silence. With further increase of the frequency-difference, and hence of the beat-frequency, the experience again changes; and there appears a sort of whir in which we can no longer distinguish individual pulses and breaks of silence, and the sound is characteristically rough. Finally, with still further increase in the frequency-difference of the tones, the rough whirring dies away more or less completely, and we are left with the two primaries.

Thus are distinguished three stages of the beat-phenomenon, appearing successively as the beat-frequency is increased from zero upwards: (1) noticeable oscillations, or surges, of intensity, (2) intermittence or pulsation of tone, and (3) roughness without intermittence. There is considerable overlapping of these stages; by no means is one strictly marked off from the next adjoining. Thus, as the beat-frequency is raised, the peculiar 'pounding' character of the second stage appears before the surging of the first stage is gone; and the

roughness which is about the only characteristic of the third stage is present throughout most of the second stage also. It is exceedingly difficult to make a judgment as to where one stage passes into the next; though out of the critical regions the experience may easily be placed in one of the three classes.

BEAT THRESHOLDS

A definition of the limits within which beats are perceptible must take into account three factors: (1) the pitch-region from which the tones are taken, (2) the intensities at which these tones are sounded, and (3) the particular criterion selected for judgment. The last-named factor follows from the above distinction of stages of the beat-phenomenon.

Adequate care in the control of the above factors has been singularly lacking in much of the work on beat-thresholds, with the result, inevitably, that the values obtained for these thresholds vary within wide limits. The earlier writers, such as Young and Robison, since they suggested that beats act as ordinary waves of sound, and pass into tones as soon as they reach an audible frequency, have been interpreted as placing the upper limit of beat-frequency in the neighborhood of 30/sec.⁸ But Helmholtz,⁹ with some temerity, placed the upper limit as high as 132/sec., which agrees fairly well with the value of 128 obtained by Koenig.¹⁰ Mayer, however, got as high as 266.¹¹ Wundt, on the other hand, insisted on a figure as low as 60/sec.¹² Finally, Stumpf,¹³ using high tones, found the limit to lie between 427 and 512; and there the matter rests today.

Values for the lower limit of frequency vary quite as much. Helmholtz¹⁴ spoke of hearing beats down to 1.5 per sec., but Mayer¹⁵ got 1 in 8 sec., Rayleigh¹⁶ 1 in 24 sec., and

⁸ See Young and Robison, *opp. cit.*

⁹ H. Helmholtz, *Sensations of tone*, 2d. Eng. ed., tr. by Ellis, 1885, p. 171.

¹⁰ R. Koenig, *Ann. d. Phys.* (2d series), 1876, 157, p. 222.

¹¹ A. M. Mayer, *Phil. Mag.* (5th series), 1894, 37, p. 283.

¹² W. Wundt, *Grundzüge der physiologischen Psychologie*, 2d ed., 1880, I., i, p. 405.

¹³ C. Stumpf, *Tonpsychologie*, 1890, II., pp. 461 f.

¹⁴ Helmholtz, *loc. cit.*

¹⁵ Mayer, *op. cit.*, p. 175.

¹⁶ Lord Rayleigh, *Phil. Mag.* (5th series), 1882, 13, p. 343.

Lindig,¹⁷ using heavy tuning forks, reported beats as slow as 1 in 3 min.

The Effect of Intensity.—The essential significance of the matter of the intensities of the primary tones seems not to have been fully appreciated in the early work on beat-perception, though it was observed by Koenig,¹⁸ and perhaps by others also, that the best beats are obtained when the primaries are of about the same intensity. The relative intensity of the two tones is the most important factor here; though the matter of absolute intensity has a bearing also. The significance of relative intensity follows from the phenomenon of the masking of one tone by another; and from the experimental findings on masking one can infer the effect of different relations of intensity on the audibility of beats.¹⁹ Any considerable disparity of intensity between two tones close enough in frequency to beat will impair the perceptibility of the beats, or will obliterate the beating completely. And, as a corollary, the least interference will occur, and hence the best beats will be heard, when the components are on the same level of intensity.

Further information regarding this matter of the intensive relations of the beating tones may be derived from an examination of Fig. 1, where are shown a few results extracted from some of my own observations. The tones used had frequencies of 1024 and 1025 \sim /sec., giving a beat-rate of 1/sec. The procedure during the observations was as follows. One tone, called the background-tone (1024 \sim), was sounded at a certain intensity, and the other tone (1025 \sim) was conducted to the same ear, and adjusted in intensity (by an Average Error method) so that beats were just perceptible.

The intensity at which the background-tone was sounded is given in TU (Transmission Units); the TU is a standardized logarithmic unit of sound-intensity, and is measured from

¹⁷ F. Lindig, *Ann. d. Phys.* (4th series), 1903, 11, p. 43.

¹⁸ Koenig, Ueber den Ursprung der Stöße und Stosstöne bei harmonischen Intervallen, *Ann. d. Phys.* (new series), 1881, 12, p. 340.

¹⁹ See R. L. Wegel and C. E. Lane, Auditory masking of one pure tone by another, *Phys. Rev.*, 1924, 23, 266 ff.

the threshold of the tone in question.²⁰ Similarly, the intensity to which the other tone had to be raised for beats to be just perceptible is expressed in the number of TU above its threshold; and this value is the lower beat-threshold

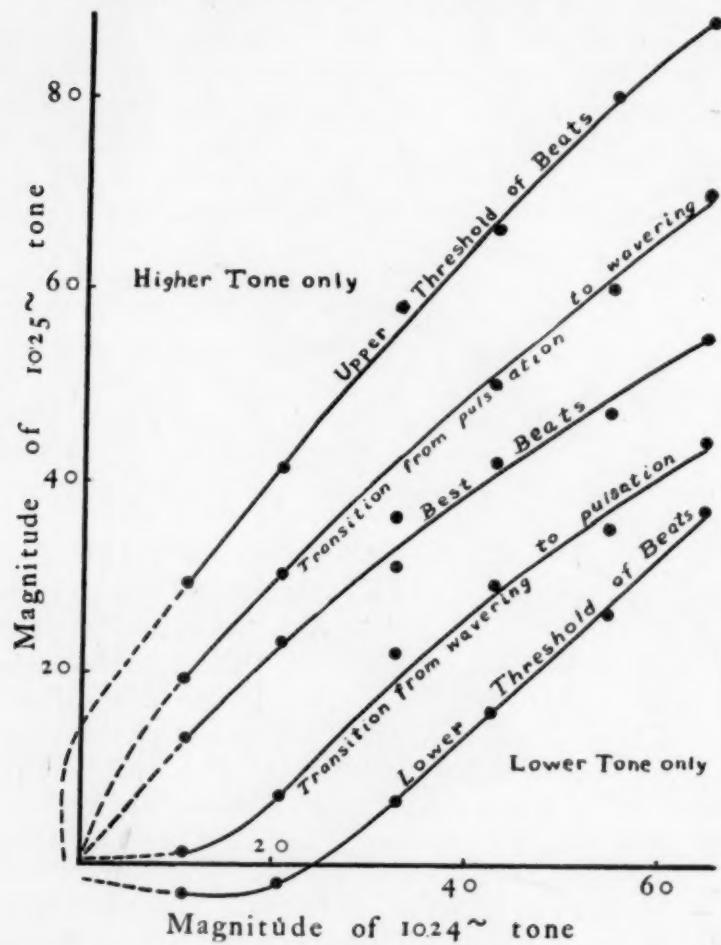


FIG. 1. The Effects of Intensity.

for the tone in the presence of the background stated. The lower curve in Fig. 1 is a plot of such values for backgrounds

²⁰ See W. H. Martin, The transmission unit, *Bell System Tech. J.*, 1924, 3, No. 3. The loudness of a sound in TU = $10 \log_{10} I_s/I_0$, where I_s is the given physical intensity (say, in ergs/sec.), and I_0 the threshold-intensity. In terms of rms pressure-amplitude in the ear-canal, the formula becomes $TU = 20 \log_{10} P_s/P_0$, where P_s is the given pressure (say, in dynes/cm.²), and P_0 the pressure at the threshold.

between about 10 and 60 TU. Values of the $1025\sim$ tone below that curve do not give beats, for this tone is then masked by the background.

If the $1025\sim$ tone be raised to an intensity far above the lower beat-threshold value, it will in turn mask the background-tone, and beats will once more become inaudible; this defines the upper threshold of beats for a given background. And the range of beat-perceptibility for two tones is the distance in TU between upper and lower thresholds as here defined. In Fig. 1 the upper curve represents the upper beat-thresholds for the different intensities of the background, while the ordinate distance between upper and lower curves at any given point represents the range of beat-perceptibility for the tones at the indicated background-intensity. If the curves be projected to about 130 TU (which is the greatest intensity that the ear will comfortably accommodate at this frequency), the area included by them will represent the complete possibilities of beat-perception for these tones.

In the figure are three other curves not yet explained. The middle one is a curve of best beats, representing for each background-intensity that intensity of the $1025\sim$ tone at which beats are strongest and most impressive. It is evident that the best beats are obtained with these tones when their intensities are nearly the same.

In the neighborhood of the middle curve the beats are characterized by the fact that the pulses are broken by intervals of complete silence. Such is not typically the case near the thresholds of beat-audibility; in the threshold-regions what one experiences is a fluctuation of sound rather than an interruption: the stronger tone continuously remains, but wavers in intensity. The two additional curves define the points of transition from pulsation to wavering of tone, as the one tone varies in intensity with a given background.

The matter of absolute intensity remains. It is shown that for low intensities of the background the lower threshold curve swings below the zero line, which indicates that here beats are perceptible at a subliminal intensity of $1025\sim$ tone.²¹

²¹ Cf. Wegel and Lane, *op. cit.*, pp. 270 f.

In the case shown the absolute intensity has no essential relevance between 20 and 60 TU; within these limits the range of perceptibility of beats is a little over 40 TU. What would happen at still greater background-intensities is difficult to say. We may expect that at high intensities the situation will become complicated by the entrance of new components, overtones and combination-tones, produced through distortion within the ear, and that these components may themselves produce beats of higher orders.

When the tones used are not pure, but contain overtones, as is true of the tones used in nearly all of the early work in the field of beat-phenomena, the effect of absolute intensity is likewise complex. The higher intensities bring out the beats of the overtones and combination-tones more strongly. And when the upper threshold of frequency for beats is being investigated, particularly with roughness as the criterion, these secondary beats become greatly significant. One might obtain high values for the upper threshold of beat-frequency representing not at all the upper limit for the beating of the primaries as such, but rather the limit for some higher order of overtones or combination-tones effective at the intensity used. And obviously the upper limit of frequency for beats will appear as a function of absolute intensity, under these conditions.

The Matter of the Criterion.—The three stages of the beat-phenomenon have been distinguished as characterized by (1) noticeable intensity-variations, (2) intermittence, and (3) roughness. As I have indicated, the work in the field of beat-thresholds has been conducted with little regard to the control of the criterion used for the judgment of beats. There is good reason for suspecting that the higher values stated for the upper limit of beat-perception have been obtained with roughness as the cue to judgment, while lower values represent the limits of the perception of intermittence. But here one cannot be sure, since all of the results are cut across by failure to control the further factors of intensity and tonal region.

I might mention a few observations of my own made

under controlled conditions with view to ascertaining in a preliminary way the limits of the three stages of beats, as defined above. The observations were all made with one tone at $1024 \sim/\text{sec.}$, while the other tone was varied from that frequency upward. In every case the two tones were equated at an intensity of about 20 TU.

Stage I. Under these conditions of stimulation very slow beats are easily observed. The rise and fall of intensity is readily noticed for beats as slow as 1 in 30 sec., and here the silent interval is very prominent. At a rate of 1 beat in 80 sec. the change of intensity is still observable, particularly when the beat is in the neighborhood of the minimum; the silent interval here is of about 8 sec. duration. With slower beats, as of 1 in 2 min., the intensity is hardly perceived as changing, though it is easy to note from time to time that it has altered; the presence of the silent interval still characterizes the experience.

If this be regarded as a beat, then I see no true lower limit of the phenomenon. I have not tried my patience with observations much below 1 beat in 2 min., but I can imagine no reason for believing that any limit inheres in the psychological situation. Rather, such a limit is imposed only by the apparatus used, and the amount of time that the observer can suitably spend in listening. I am convinced that the previous values obtained for the lower limit of perception of beats represent the limits of the apparatus used, and nothing more.

With somewhat faster beats, in the neighborhood of 1 to 6 per sec., the 'rolling' character often mentioned by previous writers is clearly evident. As the rate becomes higher the 'rolling' character is lost, and we pass into the stage of intermittence. The point of transition is roughly 6/sec., in the region of $1024 \sim$.

Stage II. About 6/sec. and higher each beat cycle is heard as a single impulse; variations of intensity within a cycle are no longer perceptible. This state of things is maintained until the rate becomes rather high, in the region of 166/sec., where the beating becomes tone-like, and inter-

mittence ceases. The experience is rather rough throughout most of this stage, and the roughness remains after the intermittence has died away.

Stage III. From 166/sec. on is the third stage, in which the experience consists of a rough tonal affair, which becomes more and more like a true tone as the frequency is increased. The roughness diminishes, and is no longer noticeable when the frequency-difference of the tones is about 356.

The Effect of Tonal Region.—The factor of tonal region may now be considered. Stumpf explained the discrepancies between the values of the upper limit of beat-perception given by different investigators as due to this factor. In support of his position he showed that the roughness of two tones differing by a given amount is greater if the tones are taken from a higher region of pitch.²²

The work of Mayer on consonance may be brought forward in support of Stumpf's position, for in that work the criterion of consonance was the absence of audible beats. Mayer showed that the separation of two tones necessary to give consonance increases with the tonal region; and this means that the higher the tone the greater is the frequency of beats just perceptible.²³

This work of Mayer's was undertaken in order to discover the relation between consonance and what we may call auditory flicker, or the perception of intensity-variations introduced into a single tone. The upper limit of frequency at which such variations can be perceived follows closely that found for consonance (*i.e.* beats), and indicates a fundamental relationship between these phenomena.²⁴

Mayer's results on both consonance and flicker are shown in Fig. 2, together with such additional observations of the upper limit of perception of beats as I have been able to assemble from the literature. As the figure shows, these data, random and occasional though they be, are consistent enough in their general trend as demonstrating that the

²² *Tonpsychologie*, II., pp. 461 *f.*

²³ A. M. Mayer, *Phil. Mag.* (5th series), 1894, 37, 275 *f.*

²⁴ Mayer, *op. cit.*, pp. 285 *f.*; *Amer. J. Sci.* (3d series), 1874, 8, 241 *f.*; *ibid.*, 1875, 9, 267 *f.* Also in *Phil. Mag.* (4th series), 1875, 49, 352 *f.*

upper threshold for the perception of beats increases with the tonal frequency.

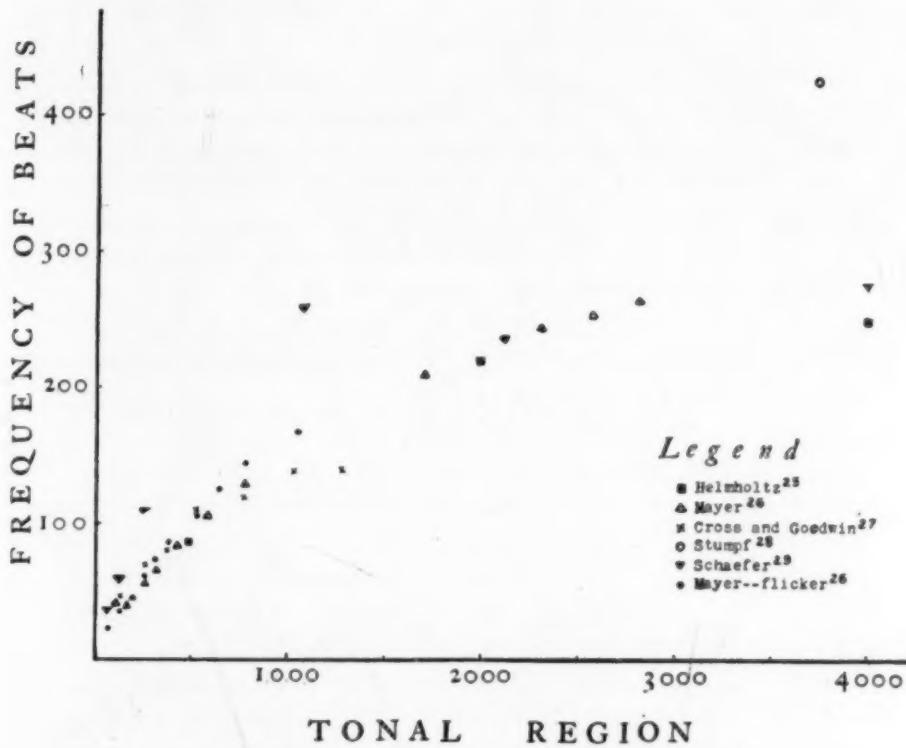


FIG. 2. Tonal Region and the Upper Frequency-Threshold.

THE BEATING COMPLEX

Some further aspects of the beat-phenomenon have now to be considered. A very old and tangled problem has to do with the pitch of the beating tone. Helmholtz reported in 1870 that under good conditions one can hear a little fluctuation of the pitch of the beating tone.³⁰ He worked out the

²⁵ Helmholtz, *Sensations of tone*, p. 171.

²⁶ Mayer, *Phil. Mag.* (4th series), 1875, 49, p. 355. A few of the observations were made by Koenig, at Mayer's request, and were reported by him, *ibid.*

²⁷ C. R. Cross and H. M. Goodwin, *Proc. Amer. Acad. Sci.*, 1891, 27, p. 4.

²⁸ Stumpf, *Tonpsychologie*, II., p. 462.

²⁹ Schaefer, in Nagel's *Handbuch der Physiologie*, 1905, III., p. 524.

³⁰ The fact, he said, had been pointed out to him by Guéroult, the French translator of his *Tonempfindungen*. See *Sensations of tone*, 1885, pp. 165, 414 f.

mathematical basis of the phenomenon, developing formulas for the pitch of the beating tone at maximum and minimum amplitudes.

Sedley Taylor discussed the subject a little later, and related the pitch-variation to dissonance in music. His derivation of the mathematical formulas agrees with that of Helmholtz, but is somewhat more complete, and he included also, as Helmholtz had not done, the case in which the two tones are equal in intensity. He reported further the results of some empirical observations, which were in accord with expectations as based upon the formulas.³¹

According to these formulas, when the two tones are equal in intensity the pitch of the beating tone lies half-way

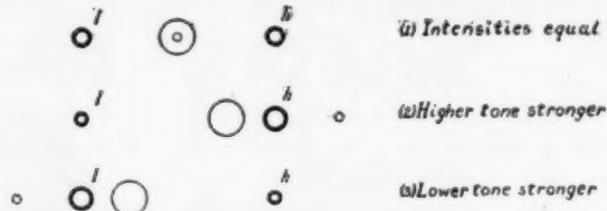


FIG. 3. The Pitch of the Beating Tone (according to Helmholtz and Taylor).

between the primaries, and does not vary. When the higher tone is the stronger the pitch oscillates between a value intermediate between the primaries and a value that is above the higher tone, the first being attained when the intensity of the beat is at maximum, and the second when it is at minimum. When the lower tone is the stronger the pitch oscillates between a value intermediate between the primaries (at maximum) and a value that is below the lower tone (at minimum). Fig. 3 will make the matter clear. The intensities are indicated by the size of the circles, while pitch is shown by position along the horizontal axis (higher pitches being toward the right). The heavy-lined circles indicate the primaries, the light-lined circles the resultant beating tone. The largest circle is the beating tone at maximum, the smallest that tone at minimum, and the pitch varies between the limits which these define.

³¹ Taylor, On variations of pitch in beats, *Phil. Mag.* (4th series), 1872, 44, 56-64.

Now it is plain that underlying the above formulas is the assumption that the two sound-waves act jointly upon a common receptive mechanism, and thus are not analysed by the ear; to the extent, therefore, that the formulas are expected to hold, we may expect a violation of Ohm's law.

Later investigators have sought to define the province within which Ohm's law holds sway. It is apparent that if Ohm's law held strictly, that is to say, if the ear acted as a perfect analyser, then we should hear no beats, and two tones led to the one ear would be perceived in a fashion essentially the same as if they had been led to separate ears. And on the other hand, if the ear analysed not at all, but responded as a quite indifferent (*i.e.* completely damped) system would do, then our perception would correspond to such a mathematical resultant of the two harmonic motions as the above formulas suggest. But the truth, according to later writers, lies between these two extremes: neither situation prevails all the time, but each arises under particular conditions. According to Bosanquet,²² at times we hear the primary tones along with the beating, and at other times the primaries are lost, and we have only a resultant tone, beating. So long as the beating rate is low, the primaries are not heard; we perceive only a tone of a pitch somewhere between the two primaries, which undergoes periodic variations in intensity. If the frequency-difference of the primaries is increased to the amount of about two commas, in the middle of the scale (for $e^2 = 660$ this amounts to $8\frac{1}{4}\sim$), the situation changes, and the primaries step in beside the beats. If the interval is further increased, beyond about a minor third (for the same tone this amounts to $132\sim$), the beats disappear, and we are left with the primaries only.

Bosanquet made no mention of the matter to which Helmholtz gave attention, the pitch-variation of the beating tone, but his findings nevertheless define the conditions under which such variations may be expected, if indeed they do occur. There is evidence that Bosanquet used tones whose

²² R. H. M. Bosanquet, On the beats of consonances of the form $k : 1$, *Phil. Mag.* (5th series), 1881, 11, 420 *ff.*

intensities were nearly equal, in which case, of course, the variations would not take place.

Stumpf's findings uphold Bosanquet. Using tones closer than a semitone (*i.e.*, probably, less than 20~ apart, as he was working with a tone in the region of $a' = 440$), he heard but one tone, beating. But with the tones $g'\#$ and a' (about 418 and 440, difference 22~), he got not only the beating tone, intermediate in pitch between the primaries, but the primaries also. When the interval was further widened, as with g' and a' (396 and 440, difference 44~), he heard the intermediate tone no longer, but only the primaries, which themselves now carried the beats.³³

But Stumpf was unable to detect the variation in pitch of the beating tone (intertone) that Helmholtz's formulas demand. When the intensities are unequal those formulas state that at the minimum of the beat the pitch steps outside the range of the primaries (see Fig. 3), and under good conditions so large a variation should be readily perceived; yet in repeated trials Stumpf was unable to discover anything of the kind.³⁴ According to his experience, the pitch of the intertone lies between the primaries, about midway with like intensities, with unlike intensities nearer to the louder; and it does not vary.³⁵

Other investigators agree essentially with Stumpf, though there is some difference as to the exact pitch-location of the tones. Meyer, like Stumpf, placed the pitch of the intertone about midway between the primaries when the intensities are equal;³⁶ Krueger, however, placed it definitely nearer the lower tone under these conditions.³⁷ In a recent discussion, Eberhardt³⁸ states the opinion that these differences are due to the failure to control the intensity-relations exactly; but her own results, except for one observer (Stumpf), are not

³³ Stumpf, *Tonpsychologie*, II, 480 *ff.*

³⁴ *Ibid.*, 477 *ff.*

³⁵ *Ibid.*, 489; and *Zsch. f. Psychol.*, 1916, 75, p. 346.

³⁶ Max Meyer, *Zur Theorie der Differenztöne*, *Zsch. f. Psychol.*, 1898, 16, p. 12.

³⁷ F. Krueger, *Beobachtungen an Zweiklängen*, *Phil. Stud.*, 1900, 16, p. 347.

³⁸ M. Eberhardt, *Ueber Höhenänderungen bei Schwebungen*, *Psychol. Forsch.*, 1922, 2, 342 *ff.*

very clear. This O , however, located the intertone about where Helmholtz's formula places it at maximum.

Eberhardt's results seem clearer on the general nature of the beating complex. So long as the difference in frequency of the primaries is not over $8\sim$, in the region of $e^2 = 660\sim$, but one tone is heard, intermediate between the primaries. It is precisely in the middle if the tones are of equal intensity; with unequal intensities it lies nearer the louder tone. With a frequency-difference above $8\sim$, but one tone, the intertone, continues to be heard if the intensities are considerably unequal;³⁹ but if the intensities are somewhere near the same, two tones are perceived. However, in this latter case it is reported that the two tones are not the primaries; one approaches closely the pitch of the louder primary, while the other lies between the primaries some $2-5\sim$ away from the first. Similar results were obtained by Hauge.⁴⁰

The evidence, considered as a whole, points to our acceptance of the following facts regarding the beating complex:

1. When the frequency-difference of the primaries is low, one does not perceive the primaries, but only one tone, whose pitch lies between the primaries. This tone is called the intertone, and carries the beats.
2. With a greater frequency-difference, the primaries step in beside the intertone, but (probably) the last continues to carry the beats.
3. With still greater difference of frequency the intertone disappears, and the primaries carry the beat, until with further frequency-difference the beating is no longer perceptible.

Regarding further points the evidence is yet inconclusive:

1. The evidence is in agreement generally that the pitch of the intertone is where Helmholtz's formula places it at maximum; but as regards Helmholtz's deduction for the beat-minimum the evidence is negative or doubtful. Later investigators do not report a variation of the pitch of this

³⁹ This is probably because the louder tone masks the other.

⁴⁰ I. B. Hauge, The application of phi-phenomena to beats, *Psychol. Monog.*, 1928, 38, No. 176, pp. 44 ff.

tone. Yet at the same time it is pointed out that the conditions are not favorable for good observation in this regard, for, in the first place, it is difficult to judge a tone when its intensity is low, and, in the second place, it is easy to confuse a small intensity-variation with a change of pitch. Hence, though in the later work there is no positive evidence for this point of Helmholtz's theory, the facts have not been interpreted as disproving it.

2. The critical interval at which the primaries as well as the intertone are perceptible is probably in the general region of $8 \sim/\text{sec.}$, in the middle of the scale (Bosanquet, about $8\frac{1}{4}\sim$ in the region of 660; Eberhardt, about $8\sim$ around 660; Stumpf, below $20\sim$ in the region of 440), but we need more exact data here. And this value should be determined at a number of points along the frequency-scale.

3. The critical point at which the intertone drops out we can designate but roughly (Stumpf, about $44\sim$ in the region of 440; Bosanquet, $132\sim$ in the region of 660).

4. Eberhardt and Hauge report, on the basis of rather limited data, that when two tones are heard these are not the primaries, but lie between the primary frequencies, differing from these by two or three cycles each.

Regarding the last four points we are in great need of more concrete knowledge.

[To be concluded]

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THE INTERPRETATION OF THE CORRELATION COEFFICIENT¹

BY ROBERT CHOATE TRYON

University of California

Introduction.—Tremendous labor and importance are given to the computation of correlation coefficients in the psychological research of today. Yet after a coefficient, r_{xy} , has been secured between two variables, X , Y , no one seems to know what it means. What one really wants to discover from the coefficient is how much one variable, say Y , is determined in percentage by X —and vice versa. Heretofore, loud cautions have been voiced against such an interpretation.

In the following pages, however, the methods will be given whereby by knowing r_{xy} , one may discover the per cent (or the limits of this per cent) by which Y is determined by X , and also the remaining per cent Y is determined by residual factors *other than* X which make up the rest of Y . It will appear that in certain common special cases these two percentages are derived from the simple coefficient, r_{xy} ; in the general case, however, another coefficient is necessary. The conditions under which these methods may be used will be stressed. Some portions of these methods have been used by other writers, who have, however, confined their analyses to certain special cases of the general principles here presented, or who have been involved in other issues generally irrelevant to our theme. Few of these writers have taken cognizance of each other's work and none has presented a comprehensive method for interpreting the correlation coefficient. The works of these writers, in particular of Nygaard, Kelley, Hull, Wright, and Spearman, will be related to these principles in the proper place.

¹ For advice and criticism on some of these matters, I am especially indebted to Dr. E. B. Wilson of Harvard, and to Dr. H. E. Jones of the University of California. The investigation was carried out on a grant from the National Research Council.

A given variable, Y , will obviously be completely determined by a composite of all the elements which comprise it, and it will correlate unity (1.00) with such a composite. Now, the *degree of determination* of Y by a component variable X , which wholly or in part influences Y , will be defined in the first section (I) as *the per cent of this total perfect (unity) correlation of Y with its complete composite which r_{xy} alone contributes*; and in section II, the degree will be defined as *the per cent of the total variance (σ^2) of Y which the variance of X alone contributes*. But these two definitions, it will appear, will lead us to the same goal of interpretation.

I. INTERPRETATION OF r_{xy} AS A DEGREE OF DETERMINATION OF Y BY X , EXPRESSED IN TERMS OF THE PER CENT OF TOTAL POSSIBLE (PERFECT) CORRELATION WITH Y WHICH X ALONE CONTRIBUTES

(A) The special case in which Y is determined by the *entirety* of X together with residual factors besides X which make up the rest of Y : This special case is introduced here because of its common occurrence, its use as a limiting case with which to clarify definitions and to introduce the more general principles to follow, and because of its having been erroneously used by some writers as a general case with which to interpret coefficients.

Conditions and formulae: Let it be given in this special case that r_{xy} has been secured between variables X and Y , and let it be understood *here* that Y is determined by the entirety of X and by other unknown and unmeasured variables. These last unknown variables we shall lump into one residual variable called D_y . These conditions may be further elucidated by saying that Y is here completely determined by X and D_y , but that all we have on hand is X ; in orthodox symbols r_{xy} is what one has actually obtained in an experiment, and r_{d_yx} is what one would expect to find between Y and another residual variable, D_y , which is the remainder of Y not in X . Since X and D_y are independent of each other, though together they make up Y , then, obviously, $r_{d_yx} = 0$.

The question is this: knowing r_{xy} , can one discover $r_{d_y y}$, the residual correlation, even though the variable D_y is not at hand? Surely, if we know these two correlations, r_{xy} , and $r_{d_y y}$, then we know the complete composition of Y , and we may secure the per cent of Y determined by X alone in terms of correlation.

This question is answerable, for knowing r_{xy} , then $r_{d_y y}$ may be easily determined from the following relation:

$$r_{xy}^2 + r_{d_y y}^2 = 1 \quad (1)$$

Before this relation is given a simple proof, let us examine it for the shedding of light on r_{xy} , the found correlation which we wish to interpret. If one consider that the entire constitution of Y be unity (1.00 or 100 per cent) and desire a scale ranging between 0 to 1.00 (0 to 100 per cent) on which the relation, r_{xy} , may be placed relative to the participation of X in Y , then r_{xy}^2 is the sought value on such a scale, for $r_{d_y y}^2$ will make up the remainder of the scale not supplied by r_{xy}^2 . Both of these values are obtainable, for r_{xy}^2 is, obviously, the square of the actually calculated r_{xy} , and $r_{d_y y}^2$ is simply $1 - r_{xy}^2$.

The degree of determination of Y by the known variable X , this degree being *defined* as the per cent of the total amount of correlation possible with Y , namely 1.00, which variable X alone produces, is in this case r_{xy}^2 ; and the degree of determination of Y by residual variables besides X which make up the rest of Y , being defined as the remaining per cent of the total possible correlation, is in this case $r_{d_y y}^2$, or $1 - r_{xy}^2$. The first power of the coefficient, r_{xy} , is not the degree of determination of Y by X as defined, because when it is added to the residual correlation, $\sqrt{1 - r_{xy}^2}$, the total is not unity. Now, when variable X is discovered to contribute, say, one half of the total amount of correlation possible with Y , that is, in this special case, where $r_{xy}^2 = .50$ ($r_{xy} = .71$), we say, for brevity of discussion, that X is one-half of Y , but meaning by this that X determines (as defined) one half of Y .

Herewith is given a simple proof of formula (1): Since X

and D_y together completely produce Y , then the multiple correlation between Y and a composite of X and D_y is unity. That is:

$$R_{y(x, d_y)} = \sqrt{1 - [(1 - r_{xy}^2)(1 - r_{d_y x}^2)]} = 1$$

Squaring both sides, and recalling Kelley's coefficient of alienation, $k_{xy}^2 = 1 - r_{xy}^2$, then

$$1 - \left\{ k_{xy}^2 \left[1 - \left(\frac{r_{d_y x} - r_{d_y x} r_{xy}}{k_{d_y x} k_{xy}} \right)^2 \right] \right\} = 1$$

But $r_{d_y x} = 0$, so that $r_{d_y x} r_{xy} = 0$, and $k_{d_y x} = 1$, hence

$$1 - k_{xy}^2 + r_{d_y x}^2 = r_{xy}^2 + r_{d_y x}^2 = 1$$

Knowing r_{xy} , we have all the necessary information in this case to discover the constitution of Y , for r_{xy}^2 is the degree of determination by X alone, and $1 - r_{xy}^2$ (or $r_{d_y x}^2$) is the degree of determination by all the other factors besides X that produce Y . The correlation coefficient of Y with these residual factors is $r_{d_y y}$ and is simply $\sqrt{1 - r_{xy}^2}$, or k_{xy} .

Besides the value of k_{xy} being the residual correlation coefficient, the expression $1 - k_{xy}$ has frequently been used as an interpretative index of the value of r_{xy} for prediction, it being compared with the value for prediction from X to Y when the correlation coefficient, r_{xy} , equals zero. Such a concept is difficult to grasp, however, and in fact it has been criticized by Holzinger (1928, p. 167) as being quite arbitrary *per se*, for he notes the possibility of a function of k_{xy}^2 being as valid an index of prediction as a function of k_{xy} . But according to our reasoning above, both k and k^2 have simple and definite meanings in this special case, k being the residual correlation, and k^2 being the degree of determination of Y from residual factors other than X . But the use of formula (1) is limited to this special case, and so is the use of k and k^2 .

Related observations on this special case: By a rather arduous algebraic derivation, Nygaard (1926) gives formula (1), and likewise presents a 'ratio of dependence' of Y on X and on D_y . He shows that where Y is completely determined 37 per cent by X and 63 per cent² by D_y , then $r_{xy} = .50$. Unfortunately, he reasons

² Nygaard's percentages have a complicated meaning which will be discussed later.

from correlation coefficients *back* to dosages of factors in the variables correlated. He says that "a correlation of .50 between mathematics and intelligence would be interpreted to mean that 37 per cent of a person's ability to master mathematics is due to intelligence, and 63 per cent to other factors" (p. 91). To be correct, his interpretation must wait upon the knowledge that there are other residual factors that produce mathematics *besides* intelligence. That is to say, he implies a cause and effect relation, for he *assumes* that *intelligence plus residual factors* produce mathematics. But one may legitimately disagree with this structure of variables by hypothesizing the opposite, namely, that a correlation of .50 between mathematics and intelligence would be interpreted to mean that 37 per cent (using the percentage notion as Nygaard does) of a person's intelligence is due to mathematics ability, and 63 per cent is due to other factors; and further, *no other factors* than those present in intelligence produce mathematical ability.

The following rough diagrams illustrate these opposing interpretations:

$$Y = X + D_y = \text{MATHEMATICS ABILITY}$$

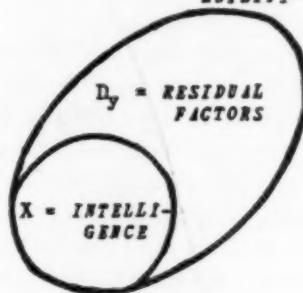


FIG. 1

$$Y = X + D_y = \text{INTELLIGENCE}$$

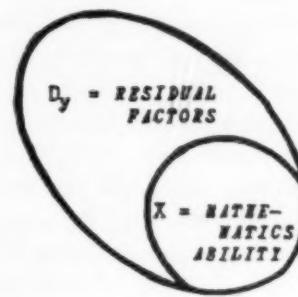


FIG. 2

In both figures, $r_{xy}^2 + r_{dy}^2 = 1$. Using Nygaard's example, let $r_{xy} = .50$. This means, using our degree of determination percentage notion, that with reference to the total possible correlation with Y , 25 per cent of that correlation is produced by X (i.e., $r_{xy}^2 = .25$), and 75 per cent, by D_y ($r_{dy}^2 = .75$). Fig. 1 illustrates Nygaard's reasoning, in which it is assumed that the entire oblong represents mathematics, Y , which is composed of the entirety of X , or intelligence, together with residuals, D_y . He reasons that this structure of variables is true *because* r_{xy} is .50. But the opposed reasoning, illustrated by fig. 2, is equally as valid.

Here, intelligence, Y , is composed of mathematics, X , and residual factors, D_y , this structure being equally as plausible if no other facts are available than the coefficient, $r_{xy} = .50$. Indeed, this interpretation is possibly more plausible, especially since most intelligence tests contain big blocks of items which definitely measure mathematical ability. The writer is not here espousing either interpretation, for even other interpretations, as we shall see later, may be given when all that one has at hand is r_{xy} .

T. L. Kelley (1919) has observed formula (1) but in its interpretation, he has reasoned similarly to Nygaard. He designates by x_1 , the variable *vocation* and "by x_2 intelligence test position and by x_3 position in a composite of *all the other factors* which determine an individual's selection of a vocation besides intelligence" (p. 60). That is, x_3 are residual factors which with x_2 completely make up x_1 . "Since x_3 comprise factors *other than* $x_2 \dots, r_{23} = 0$ and $r_{13} = \sqrt{1 - r_{12}^2} \dots$ This (r_{13}) is the correlation between the 'other factors' and vocational choice" (p. 61). Kelley believed, further, that "analysis would throw light upon what constitutes this very large field which is not intelligence but which correlates with vocational selection to the extent of .875" (p. 61). He had found that $\sqrt{1 - r_{12}^2} = .875$, where r_{12} was .484.

This interpretation by Kelley implies also a cause and effect relation to which the correlation coefficient r_{12} alone gives no support. Indeed the reverse interpretation may be given to that of Kelley's and with plausibility, to wit, that *no other factors than those in intelligence determine vocational choice*, and that 'other factors' than *vocational choice* determine and correlate .875 with intelligence. If this last interpretation is true, then a zealous hunt for 'other factors' than those in intelligence which make for vocational choice would be fruitless, for no such factors exist. Neither interpretation may be given as the truth until the assumptions as to the structure of the traits are found to be true one way or the other.

On reading the extraordinary paper by Sewall Wright (1921), one will find a complete corroboration of the position that the use of r_{xy}^2 and k_{xy}^2 imply causation.³ Where the effect, X , is produced

³ Though the reasoning and derivations as presented in the writer's paper were worked out before his having perused Wright's paper, it became immediately apparent on the reading of it that Wright's observations touch some of those presented in the present paper. I note that he uses the term *degree of determination* for the same mathematical expression as here used, though he does not give it the complete interpretation, defining it simply so: "the coefficient of determination of X by A , d_{x_A} , measures the fraction of complete determination for which A is directly responsible

by one known cause, A , and by unknown residuals, O , then for "determination in terms of correlation" . . . "for a single cause and effect the required formula is . . . r_{za}^2 . The degree of determination by residual factors is . . . $1 - r_{za}^2$ " (p. 571) or k_{za} . Thus these formula, though extremely valuable for interpretation are bounded by the conditions of cause and effect, and the composition of the variables X and Y must be *known* to bear the relation of element to composite, or it must be overtly *assumed* that such a causal relation exists.

Attention must be called to the fact that Hull's dictum, "half of a perfect correlation is not .50 but .709; the third of a perfect correlation is not .333 but .578; the fourth of a perfect correlation is not .25 but .50, and so on" (1923, p. 404) applies in practice only to the special case now being considered. He showed that if a composite test is made up of n elemental tests, each equal in correlation with the composite but independent of each other, then the correlation coefficient, r , or each elemental test with the composite is $r = 1/\sqrt{n}$, or $r^2 = 1/n$. Hence, where each elemental test is one-half ($n = 2$) of the total test, then $r^2 = 1/2$; where one-third ($n = 3$), then $r^2 = 1/3$, etc. Thus, under such a structure of the composite test, r^2 denotes the per cent of perfect correlation which the elemental test alone contributes. Observe, however, that each component test is assumed to contribute itself entirely to the composite, call it Y , and is in virtue the same as our X where the remaining elemental tests are lumped together as the residual variable,⁴ D_y . Hence, one may use Hull's dictum only in view of the assumptions involved in this special case. That is, the square of a given correlation coefficient has Hull's meaning of per cent of a perfect correlation only in the case where X is assumed or known to contribute itself to Y and as an element of it.⁵

in the system of factors" (p. 562). We shall discuss his *path coefficient* later. Those who desire systematically to determine the fractional influence of each variable of a system of known component variables on their composite should see Wright's paper.

⁴ In passing, the writer wishes to point out that using Hull's $r = 1/\sqrt{n}$ for the correlation of the elemental test with the composite implies another formula, namely, that of all the residual ($n - 1$) tests as one variable, D_y , correlated with the composite, Y . This residual correlation coefficient is k where $r^2 + k^2 = 1$. Since $r^2 = 1/n$, then $k^2 = (n - 1)/n$. Hull desired his readers to compare his $r = 1/\sqrt{n}$ with Kelley's k in a general way, and did not point out that in his notation $k = \sqrt{(n - 1)/n}$.

⁵ Interesting though not essentially quantitative are the remarks of de Ballore (1926). He notes that, in observing three correlation scatter-plots giving coefficients of .92, .69, .31, the *squares* of these coefficients give the best "psychological impression"

Uses of this special case: For cases in which the variables are known or assumed to satisfy the conditions of this case, the use of r^2 , k^2 , and k will completely disclose the constitution of the variables X and Y . Valid and important uses of these formulæ are, to give several examples, in those instances where one wishes to know the degree to which a test element influences the test composite or criterion of which it is an element, or, again, where one wishes to know the degree to which a given trial or stage of learning influences the total learning performance of which it is a part. There are many other common correlational situations, of course, in which it is known that one variable is an element of another, and to these situations the formulæ apply.

Especially is r_{xy}^2 the value to *plot* in these cases. The writer has had occasion, for instance, to plot the correlation of errors made at successive stages of maze-learning of a group against total learning performance covering all stages. Plotting first power r 's would have given an erroneous plot of successive determinations, whereas a plot of the r^2 's showed immediately the line graph of the degree to which total learning was determined by each successive stage (Tryon, 1928).

In many cases we may *assume* on the basis of *a priori* reasoning that the variables fit into this special case and that these formulæ are applicable. In correlations of speed with accuracy, for example, time appears to be a Y variable, of which errors, X , are an element. One may logically assume that total time of performance may be analysed as a variable consisting of X , time produced by the making of errors, and D_y , time occasioned by other factors, such as, in the case of rats learning a maze, timidity, speed of coördination, etc. Application of these formulæ by *assumption* should be fruitful in leading one to further experimentation in order to ascertain which variables are elementary and which contain residuals, and further, what these residual factors are.

as to the amount of relationship existing than the first power coefficients; "les nombres 0,85; 0,47; 0,10 correspondent mieux à l'idée «commune» de corrélation forte, faible, nulle . . . que les nombres 0,92; 0,69; 0,31;" and he proposes "de choisir r^2 de préférence à r , pour mesurer la corrélation."

(B) The general case in which Y is determined by a portion of X , that is, in which X and Y are related by virtue of a common variable, C .

Under the foregoing special case, we have considered that Y contains all of X plus D_y . If not all, but a portion of X participates in Y , the determination of Y by X becomes more complicated. This fraction of X , denoted by the variable C , is common to X and Y after the fashion roughly shown in the following diagram:

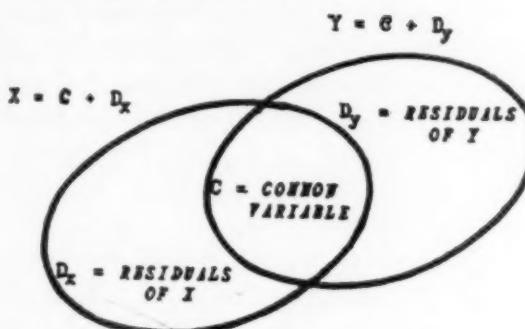


FIG. 3

In Fig. 3, X (the lower large oval) is determined by the common variable C and a residual variable, D_x ; Y (the upper large oval) is determined also by C and its unique residual variable, D_y .

Now, it must be apparent that the special case discussed under Section (A) was the limiting case in which variable X was variable C , and the degree of determination of Y by X was for that case r_{xy}^2 . But where there exists a residual variable, D_x , then no longer is the number, r_{xy}^2 , the degree of determination of Y by X (through C).⁶ Consider a

⁶ We have already seen that Nygaard's r^2 and percentage equivalent of r may be used for a reverse interpretation to the one to which he put them even in the special case to which they alone apply. His method, as we shall see, is useless where such a structure as that shown in Fig. 3 exists. From his paper the impression was given, it seems to me, that he invests his formulae with a general value not belonging to them, for he says (p. 88) that his dependence ratio is "the percentage of dependence of," using our notation, Y on X , "or more generally, the percentage of *mutual dependence* between" X and Y (italics mine). This last phrase seems to signify that he thought his formulae could be used even for the case in which common factors occasion the relation between X and Y .

construction of variables, for instance, in which 50 per cent of the correlation with Y is produced by D_y (i.e., $r_{d_y^2} = .50$), then the remaining 50 per cent must be contributed by C (i.e., $r_{cy^2} = .50$). Now, this relation may be true and yet the residuals in X , namely, D_x , may be large or small. Obviously, the larger they become, the smaller must r_{xy^2} become. Hence r_{xy^2} may no longer be the degree of determination of Y by X (through C). There exists, however, a general expression for the degree of determination of Y by X ; even though a common factor operate, one may still determine the per cent of total possible correlation with Y produced by X (through the common factor, C), and by residuals, D_y . These two degrees of determination are obtained from the following relation:

$$\frac{r_{xy^2}}{r_{cx^2}} + r_{d_y^2} = 1 \quad (2)$$

The term r_{xy^2}/r_{cx^2} is the required degree of determination of Y by X through the common factor, C . It is immediately apparent that the special case discussed under Section (A) results from X being C , for in that case $r_{cx^2} = 1$, so that formula (1) results. And only in that special case, therefore, is r_{xy^2} the degree of determination of Y by X , and $1 - r_{xy^2}$ the residual determination. Where r_{cx^2} is less than unity, that is, as residuals in X play a rôle in X , then formula (1) no longer holds, but the general formula (2) must be applied to discover the proper degrees of determination. Where a common variable is present, therefore, as in Fig. 3, then:

$$r_{xy^2} + r_{d_y^2} < 1$$

If r_{xy^2} is used regardless of the quantity r_{cx^2} (where $r_{cx^2} < 1$) then r_{xy^2} is always *lower* than the proper degree of determination of Y by X , and $1 - r_{xy^2}$ is *higher* than the residual determination. To illustrate, say that one has decided in all cases to use formula (1). And say that in one experimental incident one has discovered that $r_{xy^2} = .20$, but that r_{cx^2} , though not found, was really equal to .40. Using formula (1) one would have erroneously secured .20 as the degree of determination of Y by X , and .80 by D_y . But the

truth would only appear by the use of formula (2), and with that, one would have discovered that the determination of Y by X was truly $r_{xy}^2/r_{cx}^2 = .20/.40 = .50$, and that the residual determination was .50.

Formula (2) is a general formula to be used in all cases where a common factor occasions the relationship between X and Y , and as we have seen, it also includes formula (1) as a special case. But it requires the securing of the additional term r_{cx}^2 . Before considering how this value may be secured, let us turn to the derivation of formula (2) and of other terms which will enable one to secure the degrees of determination of both X and Y by the common variable and by their respective residuals.

Proof of formula (2):

Note that the determination of Y by X is the same as that by C , for X determines Y only through its portion C . Now, from formula (1) we see that:

$$r_{cy}^2 + r_{d_y y}^2 = 1$$

But the term, r_{cy} , may be secured from r_{xy} by holding D_x constant, that is, $r_{cy} = r_{xy, d_x}$, so that the above becomes

$$r_{xy, d_x}^2 + r_{d_y y}^2 = \left(\frac{r_{xy} - r_{d_x x} r_{d_y y}}{k_{d_x x} k_{d_y y}} \right)^2 + r_{d_y y}^2 = 1$$

But $r_{d_y y} = 0$, hence $r_{d_x x} r_{d_y y} = 0$, and $k_{d_y y} = 1$. Also observe that $k_{d_x x}^2 = 1 - r_{d_x x}^2 = r_{cx}^2$. Finally then, this reduces to:

$$\frac{r_{xy}^2}{r_{cx}^2} + r_{d_y y}^2 = 1 \quad \text{which is formula (2)}^7$$

⁷ A better proof having the merit of being initially independent of formula (1) was originally worked out by the writer, but being longer was not produced in the text. The skeleton of it is as follows:

From Fig. 3, it is apparent that Y is completely determined by C and D_y . But C may be determined from X and D_x . Hence the multiple correlation between Y and a composite of D_y , X , and D_x equals 1. For brevity, give the variables numerals, to wit: $X(1)$, $D_x(2)$, $D_y(3)$, $Y(4)$, $C(5)$. Then

$$R_{4(1, 2, 3)} = 1 = \sqrt{1 - k_{14}^2 k_{24-1}^2 k_{34-12}^2}$$

Squaring, and representing a few k^2 's by their $1 - r^2$, we get

$$1 - k_{14}^2 (1 - r_{24-1}^2 - r_{34-12}^2 + r_{24-1}^2 r_{34-12}^2) = 1$$

Reducing all coefficients to zero-order r 's, setting the zero-order expression of r_{24-1}^2 equal to some constant, say B , recalling that r_{13} , r_{23} , r_{34} equal zero, then, after manipulation one will arrive at formula (2).

Important variations of formula (2):

$$r_{cy}^2 = \frac{r_{xy}^2}{r_{cx}^2} \quad \text{Determination of } Y \text{ by } C \text{ (or } X) \quad (3)$$

$$r_{cx}^2 = \frac{r_{xy}^2}{r_{cy}^2} \quad \text{Determination of } X \text{ by } C \text{ (or } Y) \quad (4)$$

$$r_{dy}^2 = 1 - r_{cy}^2 \quad \text{Determination of } Y \text{ by } D_y \quad (5)$$

$$r_{dx}^2 = 1 - r_{cx}^2 \quad \text{Determination of } X \text{ by } D_x \quad (6)$$

The correlation between X and Y has the important composition, from (3) or (4):

$$r_{xy}^2 = r_{cx}^2 r_{cy}^2 \quad (7)$$

hence

$$r_{xy} = r_{cx} r_{cy} \quad (8)^8$$

These formulae show that when one has determined r_{xy}^2 , if he has only one determination, say r_{cx}^2 , then he may solve for *all* of the other determinations, that is, he may arrive at a complete analysis of the constitution of the two variables X and Y , thus showing the degrees of determination of each by the common factor, and each by its respective residual factor. By correlational analysis alone, therefore, he may arrive at the true cause and effect relationships.

But the 'catch' in this method is the demand for a correlation of either X and Y with the common factor, C . Before taking up briefly how this correlation may be actually secured in many, possibly most, cases, we will consider the meaning of the correlation coefficient, r_{xy} , under certain hypothesized and interesting special cases of the structure of variables.

When one has secured a given correlation coefficient, one may analyse it as follows: (1) The *highest* determination of Y by C is when Y is C . Here $r_{cy}^2 = 1$; there are no residuals

⁸I have found several references to formula (8). Wright (1921, p. 565) says that "in those cases in which the causes are independent of each other, the correlation between two variables equals the sum of the products of the pairs of path coefficients which connect the two variables with the common cause." In our case the causes are independent and "the path coefficient equals the coefficient of correlation between cause and effect" (p. 563). By partialling out his general g factor from two tests a and p which have only g in common, Spearman arrives at our eventual expression $r_{ap} = r_{ag} r_{pg}$ (1927, app. iii).

in Y , for $r_{d_y^2} = 0$ from formula (5), and the determination of X by C is $r_{cz^2} = r_{xy^2}$ from formula (7). (2). The *lowest* determination of Y by C is when X is C . Here $r_{cz^2} = 1$, and the desired determination is $r_{cy^2} = r_{xy^2}$ from formula (7). These two cases are those considered under (A). From them it is clear that either X or Y *may* be determined entirely by C , but *never* to a degree less than r_{xy^2} .

(3) An important special case lying between the two extremes of X or Y being C exists, one which greatly simplifies these formulæ. This is the case in which C *equally determines* X and Y , that is, in which the common variable or cause affects X and Y equally, and in which the residuals play an equal rôle. This case stipulates that

$$r_{cz^2} = r_{cy^2} \text{ and hence } r_{d_x^2} = r_{d_y^2}$$

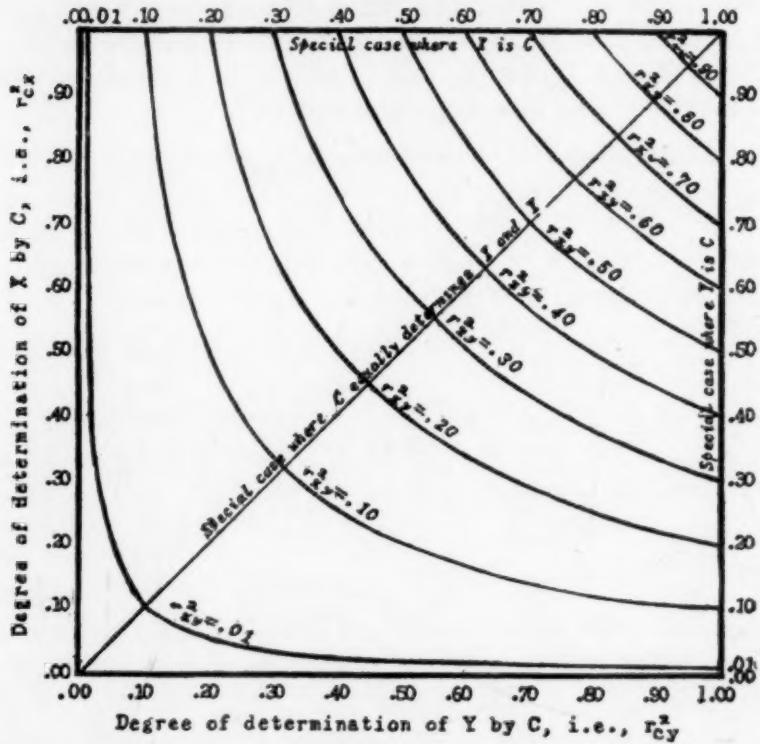
Then from formula (7)

$$\begin{aligned} r_{cy^2}^4 &= r_{xy^2}^2 \\ r_{cy^2}^2 &= r_{cz^2} = r_{xy^2} \end{aligned} \quad (9)$$

From formula (9) it is apparent that, *under these conditions*, the obtained correlation coefficient, r_{xy} , is the per cent by which X or Y are determined by the common factor, and $1 - r_{xy}$ is the per cent each is determined by residuals. Further, the *actual* correlation of either X or Y with the common variable is $r_{cz} = r_{cy} = \sqrt{r_{xy}}$, and with residuals is $r_{d_x^2} = r_{d_y^2} = \sqrt{1 - r_{xy}}$.

Chart to facilitate the interpretation of the correlation coefficient whatever the structure of variables: Fully to illustrate these cases and to aid in the analysis of any structure of variables, the accompanying chart is presented. In this chart, a given *curve* will present in its coordinates all possible interpretations which one may give to the composition of X and Y for a given value of the correlation coefficient, r_{xy} . The *ordinate* represents the degree of determination of X by C , the *abscissa*, the degree of determination of Y by C . To illustrate the function of the chart, take the case of the curve marked $r_{xy^2} = .10$. This curve covers all possible structures of variables X and Y in which $r_{xy^2} = .10$ ($r_{xy} = .32$).

The greatest ordinate for the curve is $r_{cx}^2 = 1.00$, being the case in which C entirely determines X , and hence is X ; the abscissal point for this value is at a minimum, .10, and denotes the degree to which Y is determined by C ($r_{cy}^2 = r_{xy}^2 = .10$ in this case). Note that here .90 of Y , i.e., $1 - r_{cy}^2$, is the remainder of the abscissa and is the per cent of perfect or total possible correlation with Y which is yet to be accounted for by residual variables other than X (or C in this instance). Consider the opposite structure of variables to



that just illustrated, namely, the greatest abscissal point on this same curve, $r_{xy}^2 = .10$. This point pertains to the extreme case where Y is C , hence $r_{cy}^2 = 1.00$. For this structure, the ordinate has a minimum value, $r_{cx}^2 = r_{xy}^2 = .10$, denoting that one tenth of X is determined by Y (or C in this instance) and that nine tenths of $X(1 - r_{cx}^2)$, is yet to be accounted for by residuals. As a third illustration, look

at the point where the *diagonal* cuts the curve $r_{xy}^2 = .10$. The coördinates here pertain to the middle type of structure in which the common factor, C , determines to an equal degree X and Y , for here notice that $r_{cx}^2 = r_{cy}^2 = r_{xy} = .32$. Thus for this middle case, C is 32 per cent of X and of Y , and the remaining 68 per cent of each is determined by its respective residual factors.

Though curves for r_{xy}^2 have been plotted only for successive tenths on the chart, by proper interpolation one may use it for any values of r_{xy}^2 . The important special cases where X is C , and where Y is C have been designated on it at the ordinate and abscissal point of highest magnitude respectively, and where C equally determines X and Y the diagonal is drawn. For any known structure or any given hypothesis as to the structure of variables X and Y , the proper coördinates may be immediately designated on the chart, and all the degrees of determination of both X and Y by C and by their respective residual factors may be read off.

A method of ascertaining the degree of determination of X or Y by the common variable, C : The essential point of this paper is to show that when one has secured a given correlation coefficient, r_{xy} , instead of it being uninterpretable, there is a complete percentage interpretation open. It has been assumed that the discoverer of the coefficient in most cases knows or has a tenable hypothesis as to the structure of the variables involved. But in many cases, of course, no such knowledge exists, and all hypotheses are questionable. No one knows whether X is entirely in Y , or vice-versa, or whether C equally or differentially determines X and Y . Besides the value, r_{xy}^2 , in these instances one must secure one other value, either r_{cx}^2 , or r_{cy}^2 before all the degrees of determination may be found.

Spearman (1927) has contributed the method by which, under certain circumstances, one may find the required values, r_{cx}^2 or r_{cy}^2 . This method requires securing the inter-correlations of X and Y with two other variables, Z and W , these *reference variables* having in any degree of determination the same common variable, C , present in X and Y . A very simple criterion is applied to the

inter-correlations, and if they satisfy this criterion, then one may know, with certain reservations, that the four variables are related through a common variable, and that their respective residuals are independent of each other. One may then secure the required correlation of X and Y with C .

Tetrad difference criterion: To use this criterion properly, one must know how it is composed. Let the common factors which occasion the correlation between each pair of variables be denoted as c 's with different subscripts. Then the following relation may be set up:

$$r_{zz}r_{yw} - r_{yz}r_{zw} = (r_{c_1z}r_{c_1z})(r_{c_4y}r_{c_4w}) - (r_{c_3y}r_{c_3z})(r_{c_2z}r_{c_2w})$$

where the terms in parentheses in the members on the right of the equation respectively correspond, by formula (8), to the coefficients on the left. The member on the left is Spearman's famous tetrad difference. *When this difference equals zero within its sampling error*⁹ then Spearman concludes that $c_1 = c_2 = c_3 = c_4 = C$, that is, each variable has the same common factor but has independent specific (residual) factors. By observing the member on the right, one may note that where all the c 's are the same then the difference will be zero. This common C may be present in any degree of determination in any of the variables because the tetrad difference may equal zero and the condition still hold that $r_{cz} \neq r_{cs} \neq r_{cy} \neq r_{cw}$. Now, there are two independent tetrad differences which must equal zero in order that one may be sure of a general common factor present. These are

$$r_{zz}r_{yw} - r_{yz}r_{zw} = 0$$

$$r_{zz}r_{yw} - r_{xy}r_{zw} = 0$$

The second brings in the common factors between X and Y , and Z and W , *i.e.*, c_5 and c_6 , and these must also be C for the tetrads to equal zero. There is also a third tetrad which may be neglected, since it vanishes *necessarily* when the other two vanish.

If all c 's are the same common factor, then the desired degree of determination of any of the variables by C may be immediately secured. For example,

$$\begin{aligned} r_{cz}^2 &= \frac{r_{cz}^2(r_{cy}r_{cz} + r_{cy}r_{cw})}{r_{cy}r_{cz} + r_{cy}r_{cw}} = \frac{(r_{cz}r_{cy})(r_{cz}r_{cz}) + (r_{cz}r_{cy})(r_{cz}r_{cw})}{r_{cy}r_{cz} + r_{cy}r_{cw}} \\ &= \frac{r_{xy}r_{zz} + r_{xy}r_{zw}}{r_{yz} + r_{yw}} \end{aligned} \quad (10)$$

⁹ For different methods of computing this P.E., see Spearman (1927, *appen. x*).

Criticism of the criterion: We are interested here only in finding any two variables other than X and Y which have the same common variable, C . Whether this C is universal, such as Spearman's 'g,' is not of consequence here. The opponents of Spearman's universality theory seem, in fact, to agree with him on the issue in which we are interested. S. Dodd (1928), who recently reviewed the controversy between Spearman, Thomson, Garnett, Brown, et al., says, "all the chief parties to the controversy recognize the fact that to the extent that n correlated variables are in equi-proportion (satisfy the tetrad difference criterion) they can always be factored into a uniquely determined 'g' and n specific factors, 's'" (p. 278). That this statement is true in some mathematical sense, I am not competent to judge, but certainly it may be shown that the tetrad differences may vanish even when the c 's as we have understood them in this paper are not the same common C . In fact, c_1, c_2, \dots, c_6 may be all *independent* and still the tetrad differences satisfied.

To prove this assertion, let it be *given* that between each of our four variables there is a special group factor, c ; let these c 's be independent and uncorrelated. Each of these variables is thus composed of three independent c 's which it holds in common with the three other variables. There may be residuals as well. The correlation coefficients of each variable with its three c 's must be of such several magnitudes that the sum of the squares of these three coefficients must be less than unity. For example, by formula (1)

$$r_{c_1x}^2 + r_{c_2x}^2 + r_{c_3x}^2 \leq 1,$$

if c_1, c_2, c_3 , are to be independent.

Even under these most adverse conditions, the tetrad differences may equal zero. To cite one instance, if $r_{c_1x}^2 = r_{c_2x}^2 = \dots = r_{c_3x}^2 \leq .33$, that is, if all correlations of each variable with its c 's are equal and not greater than .33, then from formula (8), $r_{xx} = r_{yy} = \dots = r_{zz}$, and both tetrad differences will equal zero. But it may be legitimately argued that such a unique structure of variables is so unlikely ever to occur that it may not be entertained as a possibility. But it is not necessary, however, for the determination of each variable by its c 's to be equal; in fact, they may be all more or less different, and the criteria still satisfied. For example, the following determinations are a case in point:

$r_{c_1z} = .30(.09)$	$r_{c_3y} = .40(.16)$	$r_{c_1z} = .40(.16)$
$r_{c_3z} = .50(.25)$	$r_{c_3y} = .30(.09)$	$r_{c_3z} = .60(.36)$
$r_{c_3z} = .60(.36)$	$r_{c_4y} = .71(.50)$	$r_{c_4z} = .68(.46)$
$r_{c_3w} = .50(.25)$	$r_{zz} = .12$	$r_{yw} = .50$
$r_{c_4w} = .71(.50)$	$r_{yz} = .24$	$r_{xy} = .18$
$r_{c_4w} = .50(.25)$	$r_{zw} = .25$	$r_{zw} = .34$

Here the degrees of determination (in parentheses) are of such magnitudes as to permit the c 's to be completely independent, and by putting the coefficients in the last two columns into the proper tetrad differences one will discover them to vanish (or to do so approximately).

Spearman's method of securing r_{cz}^2 must be used, therefore, with caution. The safest procedure would be, it seems, to select reference variables which on theoretical grounds one would *a priori* anticipate having the same common variable which associates X with Y . *Then* if the tetrad differences are satisfied, one has this additional mathematical evidence for believing in the operation of a common factor, and the r_{cz}^2 value secured by formula (10) would appear to have more validity than a value arrived at by pure assumption. But the probable error of r_{cz} seems not yet to be available—a lack which further requires one to use its computed value with caution.¹⁰

II. INTERPRETATION OF r_{xy} AS A DEGREE OF DETERMINATION OF Y BY X , EXPRESSED IN TERMS OF THE PER CENT OF THE TOTAL VARIABILITY OF Y WHICH THE VARIABILITY OF X ALONE CONTRIBUTES

The business of this section will be to show that, for all practical purposes, it is the same in principle as section I., though the approach is different. To some persons the percentage of Y contributed by X will be best thought of as the per cent of the total variability of Y which X (in general

¹⁰ The idea has suggested itself to the writer that when the tetrad differences are satisfied, the *most probable* explanation is that a common factor occasions the vanishing of them. It seems most unlikely that if group factors are at work between the various pairs of variables the criteria will be satisfied. What are needed to settle this question perhaps are formulae which express the probability of the two tetrad differences equalling zero where independent group factors only exist, and where various degrees of dependence exist. The group factor interpretation would seem

through C) contributes. We shall show here that this per cent may be found and shall prove that the value which it takes is in terms of the correlation coefficients and is in every case identical with the formula already presented in section I.

Keeping in mind Fig. 3, recall that Y is made up of the variation of C together with the variation in D_y . We desire to know what per cent of the total variation in Y is due to X (through C), and what per cent due to D_y . Denote the variance of Y by σ_y^2 . Then

$$\sigma_y^2 = m_1^2 + n_1^2 \quad (11)$$

where m_1^2 is that part of the variance of Y due to C , and n_1^2 is that part due to D_y . The value m_1 is the *effective standard deviation* of C in the production of Y ; n_1 is that of D_y . This amounts to saying that

$$y = m_1 \left(\frac{c}{\sigma_c} \right) + n_1 \left(\frac{d_y}{\sigma_{d_y}} \right) \quad (12)$$

from which by squaring both sides, summing and averaging for the group produces formula (11).

To repeat, what we desire are the following per cents:

$$(a) \frac{m_1^2}{m_1^2 + n_1^2} \text{ or } \frac{m_1^2}{\sigma_y^2}, \quad \text{and} \quad (b) \frac{n_1^2}{m_1^2 + n_1^2} \text{ or } \frac{n_1^2}{\sigma_y^2}$$

(a) being the per cent of the total Y variability which the X variable through C contributes, and (b) being the remaining per cent of the Y variability which D_y contributes.

It will now be proved that these desired per cents are actually the same degrees of determination as shown in section I., namely, that

even less probable if *four* reference variables were used and if *hexad* differences of the form $r_{xy}r_{zw}r_{ab} - r_{xz}r_{yw}r_{ab}$ equalled zero. Analogously, ogdoad and decad and even higher differences may be formed, and with the increase in the number of reference variables it seems logical that the probability of anything but a common factor operating would become less and less if these differences vanished. The writer confesses his incapacity to state what these probabilities may be and may therefore be wrong regarding them.

$$(a) \frac{m_1^2}{\sigma_y^2} = r_{ey}^2 \quad \text{and} \quad (b) \frac{n_1^2}{\sigma_y^2} = r_{dy}^2$$

Proof: Note that, employing (12),

$$\begin{aligned} r_{ey} &= \frac{\Sigma c \left[m_1 \left(\frac{c}{\sigma_e} \right) + n_1 \left(\frac{d_y}{\sigma_{dy}} \right) \right]}{N \sigma_e \sigma_y} \\ &= \frac{m_1 \left(\frac{\Sigma c^2}{\sigma_e} \right) + n_1 \left(\frac{\Sigma c d_y}{\sigma_{dy}} \right)}{N \sigma_e \sigma_y} \end{aligned}$$

But $\Sigma c^2 = N \sigma_e^2$, and $\Sigma c d_y = 0$, hence, cancelling properly,

$$r_{ey} = \frac{m_1}{\sigma_y} \quad \text{and} \quad r_{ey}^2 = \frac{m_1^2}{\sigma_y^2} \quad (13)$$

Note, also, that

$$\begin{aligned} r_{dy} &= \frac{\Sigma d_y \left[m_1 \left(\frac{c}{\sigma_e} \right) + n_1 \left(\frac{d_y}{\sigma_{dy}} \right) \right]}{N \sigma_{dy} \sigma_y} \\ r_{dy} &= \frac{m_1 \left(\frac{\Sigma d_y c}{\sigma_e} \right) + n_1 \left(\frac{\Sigma d_y^2}{\sigma_{dy}} \right)}{N \sigma_{dy} \sigma_y} \end{aligned}$$

Since $\Sigma d_y c = 0$, and $\Sigma d_y^2 = N \sigma_{dy}^2$, then

$$r_{dy} = \frac{n_1}{\sigma_y} \quad \text{and} \quad r_{dy}^2 = \frac{n_1^2}{\sigma_y^2} \quad (14)$$

Formula (13) tells us that the per cent of the variance of $Y(\sigma_y^2)$ which is due to X through $C(m_1^2)$ equals r_{ey}^2 and this value is exactly that which denotes the degree of determination of Y by X found in Section I., formula (3). Likewise, formula (14) tells us that the remaining per cent of the variance of Y due to residuals, *i.e.*, (n_1^2) , equals r_{dy}^2 , which is the degree of determination of Y by D_y found previously in formula (5). *Hence, the degree of determination of Y by X expressed as a per cent of total possible (perfect) correlation which X alone contributes is identical with the degree of deter-*

¹¹ Observe that $m_1^2 = r_{ey}^2 \sigma_y^2$, and $n_1^2 = r_{dy}^2 \sigma_y^2$. From these important relations, one may determine the amount of variance itself which the common variable and the residual variable respectively possess relative to their participation in the make-up of Y .

nation expressed as a per cent of the total variance of Y which the variability of X alone contributes. A similar identity holds for the degree of determination of Y by residuals.

Wright's path coefficient: One may raise the question, why not use m_1/σ_y , the ratio of the effective standard deviations, instead of the ratio of their squares, m_1^2/σ_y^2 , as the degree of determination of Y by X ? This first ratio is the *path coefficient* used by Wright (1921). Says he, "the path coefficient for the path from A to X will be defined as the ratio of the standard deviation of X due to A to the total standard deviation of X " (p. 562). But this ratio of first power standard deviations is not a meaningful per cent, for $m_1 + n_1 \neq \sigma_y$. On the other hand $m_1^2 + n_1^2 = \sigma_y^2$, so that

$$\frac{m_1^2}{\sigma_y^2} + \frac{n_1^2}{\sigma_y^2} = 1 \quad (15)$$

Hence the degree of determination as we have used it throughout this paper is the intelligible per cent for from relation (15) it is clear that when the determination from X is added to that from residual factors other than X which make up the rest of Y , the sum is unity or 100 per cent. But as an interesting theorem, it may be well to remember that the correlation coefficient, such as r_{cz} , means the per cent which the effective standard deviation of the common factor represents relative to the total standard deviation of Y .

Nygaard's percentage equivalent for the correlation coefficient: At this point the meaning of Nygaard's 'percentage equivalent' or 'dependence ratio' will be apparent. As was pointed out in Section I., his ratio pertains only to the special case where X or Y is C . Now, he has suggested that the dependence of Y on C be denoted (in our terminology) by the ratio:

$$d_{yz} = \frac{m_1}{m_1 + n_1} = \frac{r_{xy}}{r_{xy} + \sqrt{1 - r_{xy}^2}}$$

This ratio is the effective sigma of X divided by the effective sigma of X plus the effective sigma of D_y . As a per cent it is extremely difficult to interpret. Had the denominator

been $\sqrt{(m_1^2 + n_1^2)}$ then such a ratio would have been none other than Wright's *path coefficient* and would have been equal, in this special case, to r_{xy} .

Kelley's special case: Similarly, a formula recently used by Professor Kelley (1927) is another special case of formula (13). The special case in which he was interested is the same one mentioned above which led to formula (9), namely, the case where C equally determines X and Y . Kelley approached this formula through the medium of the standard deviations involved. Starting (p. 193) with $\sigma_y^2 = u^2\sigma_e^2 + \sigma_{d_y}^2$, and $\sigma_x^2 = w^2\sigma_e^2 + \sigma_{d_x}^2$, where, in our notation, $u^2\sigma_e^2 = m_1^2$, $\sigma_{d_y}^2 = n_1^2$, $w^2\sigma_e^2 = m_2^2$, and $\sigma_{d_x}^2 = n_2^2$, then for the special case in which the variability in the residuals D_x and D_y plays an equal rôle in determining the variability of X and Y respectively, that is, where $n_1^2/\sigma_y^2 = n_2^2/\sigma_x^2$, then

$$\frac{u^2\sigma_e^2}{u^2\sigma_e^2 + \sigma_{d_y}^2} = \frac{m_1^2}{m_1^2 + n_1^2} \text{ (of our notation)} = r_{xy}$$

For this case, therefore, "the coefficient of correlation . . . is that proportion of the total variance [σ_y^2 of our notation] which is due to the common factor [m_1^2 of our notation] present in each test" (X and Y). This fact may be most briefly derived in our notation, for in the special case $n_1^2/\sigma_y^2 = n_2^2/\sigma_x^2$, then from formula (15) $m_1^2/\sigma_y^2 = m_2^2/\sigma_x^2$. But

$$\begin{aligned} m_1^2/\sigma_y^2 &= r_{ey}^2 \text{ from (13)} = r_{ex}^2 \text{ [for this case]} \\ &= r_{ex}r_{ey} \text{ [which by (8)]} = r_{xy}. \quad (16) \end{aligned}$$

Kelley used this special case as a means of determining the per cent of common factors between intelligence (X) and achievement (Y). He had secured an r_{xy} corrected for attenuation of .90. He *assumed* that the variance of D_x is the same ratio to the total variance of X as the variance of D_y is to that of Y . Though he admitted the possibility of error in this assumption, he concluded "that 90 per cent of the two traits correlated was identical, and 10 per cent different" (p. 196), since $m_1^2/\sigma_y^2 = r_{xy} = .90$ by formula (16). Another assumption, equally valid, could be offered, namely, that achievement consists of all of the variability of intelli-

gence plus residuals. This being assumed, then from formula (3), since $r_{cx}^2 = 1.00$, $r_{cy}^2 = r_{xy}^2 = .81$, a value which means that 81 per cent of the two traits correlated are identical, and 19 per cent different. Neither of these interpretations is probably correct, but the true interpretation will wait upon one's discovery, by Spearman's method, of either r_{cx}^2 or r_{cy}^2 . This much may be said without any further work, however, that *at least* 81 per cent of each trait depends for variation or correlation upon the same factors.

III. CONSIDERATION OF ASSUMPTIONS

An implicit assumption: One assumption is involved in the use of these general formulæ presented. This assumption is not readily apparent from (2) and (15), the basic general formulæ, but it is evident from (12). The assumption is made that variable Y is composed of variable C *added to* variable D_y . But Spearman has settled the problem of this assumption as follows: "Surprise may be felt that the measurement, m_{az} (our y), even if truly enough a function of the two factors general (our C) and specific (our D_y) should so simply consist of merely the *sum* of these added together. . . . It might have been supposed to consist of the *product* of the two factors. Or it might have been any of an endless number of other and more complex functions of the two factors. The answer to this question is that our proof has depended upon usage of Taylor's theorem, according to which all mathematical functions however complex can, in general, be expressed in the above simple additive form with some approximation. This theorem has supplied the main foundation for the whole theory of correlation, from the original work of Bravais onwards; indeed, it is among the main props even of physics" (1927, *appendix xv*). Wright attacked this question empirically and found, in our notation, that even though Y is a *product* of C and D_y , the degrees of determination as represented by r_{cy}^2 and $r_{d_y y}^2$ were closely approximated (1927, p. 564).

Assumptions a priori: (a) *as to the structure of variables:* In any given analysis, where Spearman's method of finding

r_{ex}^2 or r_{ey}^2 is not used, one may assume whatever constitution of X and Y he wishes, on the basis of which the degrees of determination may be calculated. The burden of proof for such an assumed structure lies upon the experimenter who must produce the argument for such an assumed structure on *a priori* grounds. In such an event, the experimenter should make *explicit* the assumptions and the basis of them. If it is evident, however, that no respectable assumption may be made as to the structure of the variables investigated, then the wise procedure would be to make various assumptions, such as $r_{ex}^2 = 1.00$, or $r_{ey}^2 = 1.00$, or $r_{ex}^2 = r_{ey}^2$, thus entertaining the various extreme and middle possibilities so as to give some insight, at least, into the range of interpretation of the found correlation coefficient.

(b) *As to the nature of basic elements which produce the variables:* No assumption has been made in the foregoing pages as to the nature of the elements which make up the variables. In using the formulæ as presented to this point, one discovers the degree of determination of Y by X , understanding by such degree, the per cent of total (perfect) correlation, or of total variability, which X alone contributes to Y . The basic elements which cause the variation in X and D_y may be the same or different, the degrees of determination still remaining intelligible and comparable. *But if one wishes to make the assumption* that all the variables involved are made up of what Thomson (1927, p. 145) calls 'equal, all-or-none elements' . . . 'possibly neurones,' or what Kelley (1924, p. 189) calls 'independent elemental factors' such as pips on dice, then the formulæ have a percentage meaning which they otherwise do not possess. Under such an assumption, for example, in the special case of formula (1), r_{xy}^2 "is the proportion of elements determining X which are involved in Y " (Kelley, p. 190), and in the special case of formula (9) r_{xy} is "the proportion of elements common to the two traits" (p. 190). The position that one may assume, however, that the variables are basically produced by dice-like elements may in some cases be legitimately defended on *a priori* grounds, but to prove the actual operation of such elements would be difficult.

Using such assumptions as these, and arguing *from a special case*, Kapteyn (1912) of the Royal Astronomical Society sought to justify the use of the first power r as the most natural index of correlation. Kapteyn went back to Bravais' original work and gave it "the very small extension needed for getting at . . . a definition" of the correlation coefficient in such natural terms. After some extension, he finally arrived at the following: "Let

$$\begin{aligned}\xi &= m + n + \cdots \mu + \nu + \cdots \\ \eta &= m + n + \cdots \mu' + \nu' + \cdots\end{aligned}$$

and let the elements, $m, n, \cdots \mu, \nu, \cdots \mu', \nu', \cdots$ all have the same mean error. . . . Let a be the number of common elements, and b the number of independent elements, $\mu, \nu, \cdots \mu', \nu'$, *both* in ξ and η , then it seems natural to take $a/(a + b)$ as the quantity of correlation or the correlation coefficient" (p. 525). He then showed that $r = a/(a + b)$, and concluded that "the quantity r itself, and not such a function as $\sqrt{r}, r^2 \dots$ must be considered as the natural measure of correlation." Unfortunately, r is such a *natural* measure (*i.e.*, per cent of $a + b$ which a , the common elements, constitutes) *only for the special case* in which ξ and η have the *same number* of independent elements, b , as is here provided. But when ξ has b_1 independent elements, and η has b_2 of them, and where $b_1 \neq b_2$, then r is no longer the simple per cent $a/(a + b)$. In fact, r^2 is such a natural measure, $a/(a + b)$, in the limiting special case where $a = \xi$, for then $\eta = \xi + b$, and $r^2 = \xi/(\xi + b)$, as was pointed out in the preceding paragraph.

IV. SUMMARY AND CONCLUSIONS

To elucidate the meaning of the correlation coefficient, r_{xy} , between any two variables, X and Y , by interpreting it in terms of the per cent of Y determined by X , a series of formulae have been derived. The per cent of Y determined by X is called the degree of determination of Y by X . This degree is defined as the per cent of *perfect correlation* possible

with Y which X alone contributes, or as the per cent of *total variance* of Y which the variance of X alone contributes. These two definitions have been shown to be mathematically identical.

Where X influences Y in general through a common variable, C , general formulæ are given which denote the determination of Y by X , and of Y by residual factors other than X which determine the remainder of Y . The degree of determination of Y by C is for the general case equal to r_{xy}^2/r_{cz}^2 , that by residuals is equal to $1 - (r_{xy}^2/r_{cz}^2)$. To use these and related formulæ, one must either *assume* the magnitude of the coefficient, r_{cz}^2 , to be a given value, or one must *obtain* the actual value itself by using a method devised by Spearman. With this value at hand all the degrees of determination may be arrived at. Where only the method of assumption is used, then various limiting and middle special cases are available which may be used to illustrate the range within which the interpretation of r_{xy} may vary.

It is illogical to choose blindly either r or r^2 as the natural index of correlation between X and Y . The true index of the relation is the degree of determination of one of the variables by the other, for example, of Y by X . Where X is itself the common factor, then r^2 is the percentage determination of Y by X , where X and Y are equally determined by C , then r is the percentage determination of Y by X , and where Y is C , then the determination of Y by X is unity, or one hundred per cent.

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THE FREAKS OF CREATIVE FANCY

BY S. J. HOLMES

University of California

The operations of our minds are seldom revealed to our vision in any clear and distinct manner. When we day dream we may have pictures of things and events, but they are usually so dim and vague that we are not quite sure whether we see them or not. Introspection as a psychological method is notorious for its dangers. The behaviorists of course will not tolerate it for a moment. Nevertheless even a behaviorist is apt to betray the fact that he employs it whenever he is off his guard.

Occasionally our mental images assume a degree of vividness which enables us to observe them without the uneasy feeling that we may have been deceived. Such an opportunity recently fell to my lot, and my experience was of so unusual a kind, and so different from any of my other experiences before or since, that I cannot let it pass by without record. During the early part of my convalescence from an illness there was a time when I could distinctly see the outlines of my mental pictures on the walls and ceiling of my room. Being, as I believe, of sound mind and in full possession of my faculties, I watched with eager scientific curiosity the smaller details of these pictures which seemed to be almost as clearly outlined as the material objects before me. The scenes changed frequently and unexpectedly. Yet they persisted long enough for me to give them a careful and critical inspection.

In one instance I contemplated a field of round, whitish boulders. These soon transformed into a herd of sheep which began slowly to move away. Then the sheep turned to a mass of white cumulus clouds; and finally the scene ended somewhat ridiculously in becoming changed into a huge cauliflower. In another picture I saw a row of columns rolling down towards me. These were at first small, like pencils. Then they appeared like larger columns of stone; and in the end were changed to whitish pieces of asparagus similar to those that had previously come in on my tray.

More striking than these fancy pictures were the images of animals which appeared above the molding opposite my bed. In nearly all cases only the heads of the animals appeared, but the

peculiar thing about them was that they usually represented entirely new forms. One creature impressed me particularly because it had large, staring eyes situated near the tip of its nose. Its head was shaped much like that of an opossum, and its fur was light gray sprinkled with little black pencils of hairs. I wondered about these pencils of hairs because I had never seen fur of this kind, and because they seemed to stand out so distinctly.

I was keenly interested in these pictures and endeavored to ascertain to what an extent I could observe their finer details. I remember a lion with a partly open mouth. I tried to count his small incisor teeth. I think there were six of them; but I found it difficult to count objects in a series and soon became exhausted by the effort. The endeavor to explain these animal types as combinations of different forms I had previously seen was quite unsuccessful. For the most part they seemed entirely unique. There were some weird creatures among them which interested me particularly as a professor of zoölogy. I am sure that I created enough members of new orders and classes to fill a large menagerie.

After these animals had occupied the screen for some time there was a succession of human forms and faces. Among these there were some familiar ones such as Woodrow Wilson, Theodore Roosevelt, and P. T. Barnum, one of the heroes of my boyhood days. I also saw the celebrated Dr. Munyon standing with his hand and forefinger pointing upward. In the twinkling of an eye he was transformed into President Coolidge looking very solemn and taciturn. No moving picture show could have been half so fascinating.

The most remarkable exhibit of all was a group of faces of Indians which I could see upon the ceiling. I think that in no case were they like any pictures I had ever seen before. In general their faces impressed me as evincing an unusual degree of intelligence and force of character. What fine specimens of their race they seemed to be! There were at least a dozen to be seen at once, and I closely inspected one after another, noting the little peculiarities of their features, their different expressions, and the varied forms of their head dress. But what particularly surprised me about these pictures of Indians was the fact that they seemed to be such artistic products. This struck me as all the more remarkable as I have no artistic talent whatever and have never occupied myself in making pictures. Nevertheless my creative fancy was able to produce a dozen of these portraits at once, each quite different from

the others, and to do it all in what seemed to be an instantaneous act.

My gallery of Indians persisted for several minutes so that I had sufficient time to scrutinize them in detail. The production of these faces was in no sense a voluntary act. I lay and contemplated them as I would watch a play on the stage. One part of my mind was making these synthetic products, while another part was watching what was going on and speculating as to what possible neurological activities might accompany and account for the display which seemed projected upon the ceiling. I was filled with wonder at the creative power of the mind, even though it was a rather poor and weak one.

This power, which was so conspicuously brought out as the result of illness, doubtless functions to an even greater extent in health. It exhibits itself in dreams in which we create many new scenes and situations. In dreams we take things for granted and are not aware that we are dreaming. We are not struck by the absurdities of the dream experience until after we are awake. A person in delirium also has vivid fancies, but how clearly defined they are is difficult to ascertain. In the experiences I have described the fancy pictures were very clearly outlined and at the same time my mind was as clear as in health, and was actively taking advantage of the opportunity to observe some of its own peculiar behavior.

Here is a bit of psychological information gained by the method of introspection, although it seemed like the observation of external objects. What a behaviorist would make of it I cannot imagine. All that would be patent to him would be an individual lying in bed and gazing up at the ceiling. The methods of the behaviorist are quite incompetent to give any inkling of the complex play of mental processes involved in the experiences described.

We are prone to look upon the play of fancy as consisting mainly in the revival of impressions we have once experienced. But the images I could see so clearly could not be explained as due to the mere recall of previous visual impressions. They were new creations. Doubtless they were formed of elements derived originally from sense experiences, and they were put together according to patterns more or less like objects frequently seen. But when several weird-looking animals never before seen on land or sea were visible at the same time, and a dozen apparently new Indian faces, all different, seemed to be looking at me from the ceiling, it is evident

that the mind must have been doing an extraordinary amount of constructive work. Under ordinary circumstances a large part of the constructive activity of our minds escapes our notice. Most of this activity seems quite useless so far as any advantage to be derived from it is concerned. It is a kind of semi-conscious play.

Now why does the mind take the trouble to form these various constructs? What sort of cerebral mechanism can we postulate as the basis for this kaleidoscopic play of images? Each image must involve the excitation of a particular combination of neurons; in fact there must be several of these neural patterns excited at the same time in order to account for the simultaneous appearance of several images, to say nothing of the neural activities involved in thinking about them. Neural activity must have a peculiar tendency to fall into definite excitation patterns, but what should cause it to produce something quite new, like an animal's head with large staring eyes absurdly near the tip of its nose and gray fur with little pencils of black hairs, is indeed a mystery.

This power of making novel combinations of its materials is perhaps the most wonderful attribute of the mind. It works without effort or volition. One cannot plausibly explain it as a result of conditioned reflexes, however complex. It is difficult to interpret it as a phase of the process of the biological adjustment of the organism to its environment. Indirectly of course this constructiveness is useful to the organism, although its particular products in most cases can hardly be conceived to have any practical value. As to understanding the physiological basis of this creative proclivity of our minds we have not made the first step. Even a poor brain must be a very wonderful kind of a machine.

[MS. received February 11, 1929]

DISCUSSION IN REPLY TO THE RESCUER

In replying to Professor Weiss's note 'Some Succor for Professor Kuo' (PSYCHOL. REV., 1929, 36, pp. 254-255) I wish to call attention to an error of observation, reply to an argument, and comment upon a statement of fact.

Dr. Weiss writes: "Dr. Rosenow claims that he cannot get along without the concept of purpose. Of course if this is the case his argument is unanswerable." Now as a matter of observable, verifiable fact such a statement does not occur in my three hundred word note (PSYCHOL. REV., 1928, 36, p. 532). The following statement does occur. "Dr. Kuo would hold that 'attempting to recover a hat' is merely a convenient summary of physiological facts the exact nature of which we do not know. That would seem to depend upon the physiologist; there are physiologists who cannot do without the concept of purposive activity and who, accordingly, have concepts for summarizing such facts." Obviously, I was replying to Dr. Kuo's facile assumption that physiological fact, conveniently summarized or not, is necessarily free from the taint of purpose. Dr. Weiss seems to have read the name 'Rosenow' in place of the objectively present word 'physiologists' and does not seem to have observed the context of the part of the sentence to which he refers.

Dr. Weiss writes: ". . . if Dr. Rosenow limits his purposive behavior to the first class (that which is verifiable by others), it seems to me that all objections vanish and that he does not disagree with Dr. Kuo." Dr. Weiss seems to think that there is, or that some one alleges that there is, a *class* of unverifiable facts. There is no such class. There are no such facts. If we cannot prove that a given purposive act occurred, we do not know that the occurrence of that act is a *fact*. However, if Dr. Weiss agrees that purposive behavior occurs as a matter of verifiable fact, we are indeed in complete agreement upon that point.

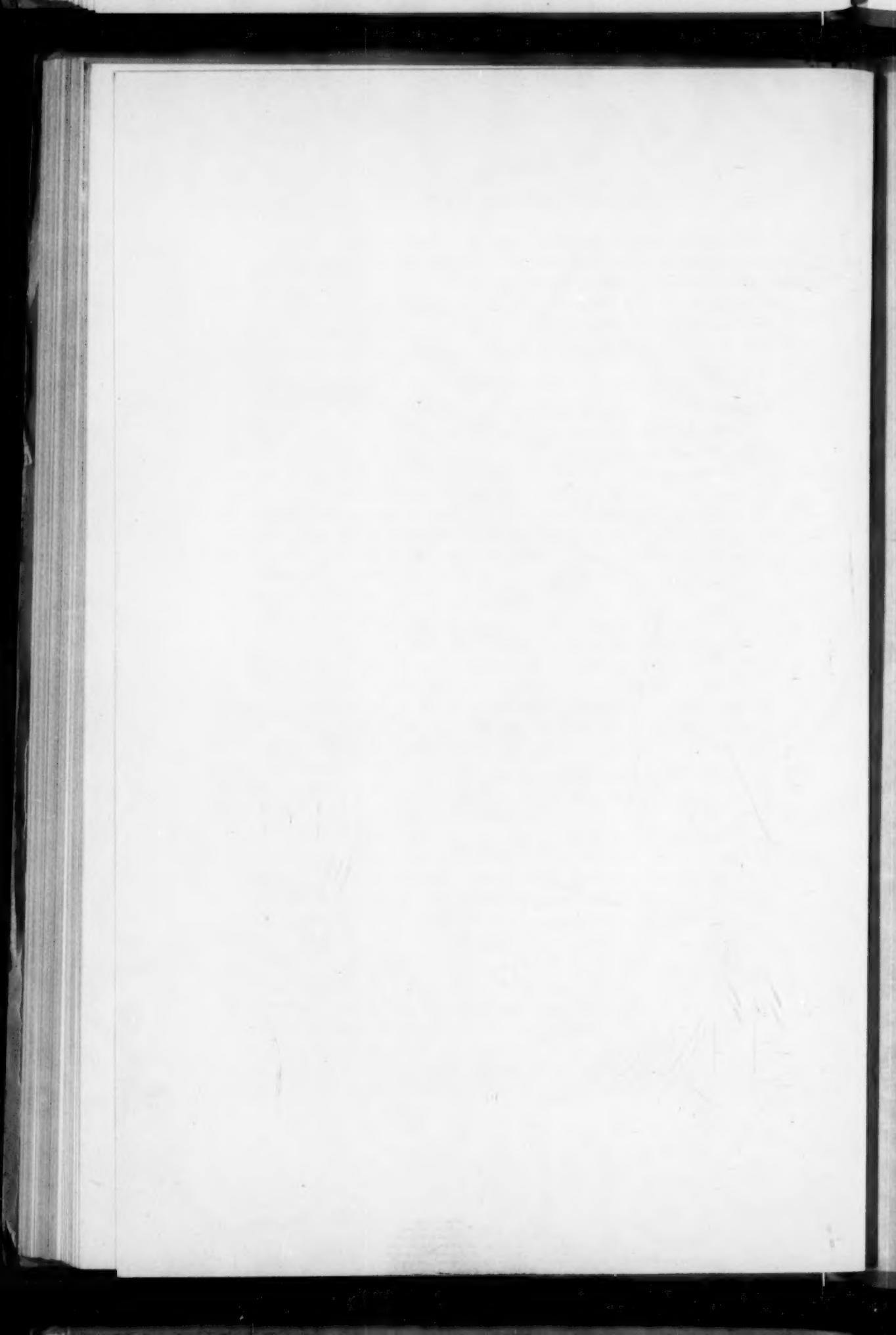
Dr. Weiss writes: "My objections to purposive concepts . . . rest upon the fact that these classifications can not be made uniform enough to be treated scientifically." He goes on to point out that

psychologists disagree vigorously about such classifications. I had supposed that vigorous disagreement amongst scientists indicated that an important problem remained unsolved, not the scientific incompetence of one side or the other. Meantime problems concerning purposive behavior have been and will continue to be treated scientifically and competently by scientists whose interests lie that way.

CURT ROSENOW

INSTITUTE FOR CHILD GUIDANCE,
N. Y. CITY

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